S. F. BAIRD.

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PROCEEDINGS

S. F. BAIRD.

OF

THE AMERICAN ASSOCIATION

FOR THE

ADVANCEMENT OF SCIENCE.

SEVENTH MEETING,

HELD AT CLEVELAND, OHIO,

JULY, 1853.

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OFFICERS OF THE ASSOCIATION

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CLEVELAND MEETING.

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Prof. S. F. Baird, *Permanent Secretary.*
Prof. J. D. Dana, *General Secretary.**
Dr. A. L. Elwyn, *Treasurer.*

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Prof. L. Agassiz,
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Dr. Joseph Leidy,
Dr. Wolcott Gibbs,
Prof. O. M. Mitchel,
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J. M. Woolsey,
Prof. H. L. Smith,
H. L. Kingsley,
O. H. Perry,
Dr. J. S. Newberry,
A. Stone, Jr.

---

* In the absence of Prof. Dana, Prof. S. St. John discharged the duties of the General Secretary, and took his place on the Standing Committee.

SPECIAL COMMITTEES.*

A. Committees continued from the Last Meeting.

1. *Committee on Annual Assessments and Tickets.*

Prof. Asa Gray,
Dr. Alfred L. Elwin,
Prof. Henry D. Rogers.

2. *Committee on the United States Coast Survey.*

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Prof. Caswell,
Prof. M. J. Williams,
Prof. Charles F. McCay,
I. W. Andrews,
Dr. L. P. Yandell,
Prof. O. M. Mitchel.

3. *Committee on a Uniform Standard of Weights and Measures.*

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Prof. Arnold Guyot,
Prof. A. D. Stanley,
Lieut. M. F. Maury,
Prof. A. D. Bache.

4. *Committee to Memorialize State Governments upon the Importance of commencing or continuing Geological Surveys.*

Dr. R. W. Gibbes,
Pres. E. Hitchcock,
Prof. H. D. Rogers,
Dr. C. T. Jackson,
Gov. J. W. Matthews,
Prof. Louis Agassiz,
Prof. B. Silliman, Sen.,
Prof. J. N. Safford,
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Prof. William B. Rogers,
J. Hamilton Couper, Esq.,
Dr. T. Romeyn Beck,
James Delafield, Esq.,
Prof. L. C. Beck,
Prof. J. Henry.

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* All these committees were discharged excepting No. 5.

5. *Committee to Memorialize the Legislature of Ohio on the Subject of a Geological Exploration of that State.*

Judge LANE, *Chairman*,
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S. MEDARY, Esq.,
Judge VANCE,
JOHN H. JAMES, Esq.,
Prof. S. ST. JOHN,
ROBERT BUCHANAN, Esq.,
JOHN P. FOOTE, Esq.,
Hon. ALLEN TRIMBLE,
Hon. S. J. ANDREWS.

6. *Committee to Memorialize the Legislature of Missouri on the Subject of a Geological Survey of that State.*

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Prof. A. D. BACHE,
Prof. JOSEPH HENRY,
Prof. LOUIS AGASSIZ,
Dr. GEORGE ENGELMANN,
Dr. H. KING,
ROBERT BUCHANAN, Esq.,
Prof. JAMES HALL,
Major M. L. CLARK.

7. *Committee for Memorializing Congress in Relation to Scientific Explorations, and the Distribution of the Duplicates from the Collection of the Exploring Expedition.**

Dr. ROBERT HARE,
Prof. BENJAMIN SILLIMAN, Sen.,
Prof. STEPHEN ALEXANDER,
Prof. HENRY D. ROGERS,
Pres. EDWARD HITCHCOCK,
WILLIAM C. REDFIELD, Esq.,
Prof. BENJAMIN PEIRCE,
Dr. ROBERT W. GIBBES,
Prof. LOUIS AGASSIZ,
Dr. SAMUEL G. MORTON.

8. *Committee to Arrange the Detail of a System of combined Meteorological Observations for North America.*

Prof. A. D. BACHE,
WILLIAM C. REDFIELD,
Prof. JOSEPH HENRY,
Capt. J. H. LEFROY,
Prof. A. GUYOT,
Dr. T. R. BECK,
Prof. A. CASWELL,
Hon. WILLIAM MITCHELL,
Prof. ELIAS LOOMIS,
Prof. J. H. COFFIN.

---

* This Committee has also been requested to consider the propriety of memorializing Congress on the subject of granting public lands to Missouri, for the purpose of carrying on a geological survey of that State.

9. *Committee to Digest a Plan of Reducing the Observations made under the Direction of the Regents of the University of New York, from 1825 to 1850, with Reference to their Publication, and to decide upon the Stations which shall be included in this Reduction.*

Dr. T. R. Beck, | Prof. A. Guyot, | Prof. E. Loomis.

10. *Committee to Memorialize the State of New York, and other States, in Regard to Geographical Surveys.*

Prof. A. D. Bache,
Prof. O. M. Mitchel,
Prof. E. Loomis,
Prof. W. M. Gillespie,
Samuel B. Ruggles, Esq.,
Prof. C. Hackley,
Lieut. E. B. Hunt.

B. New Committees Appointed.

Committee to Revise the Constitution.

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Prof. J. Lawrence Smith,
Prof. John Leconte,
Dr. Wolcott Gibbs,
Dr. B. A. Gould, Jr.,
Prof. W. B. Rogers,
Prof. J. D. Dana,
Dr. Joseph Leidy,
Prof. S. S. Haldeman,
Dr. A. A. Gould.

Committee to Memorialize Congress in relation to a Geographical Department of the Congress Library.

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Gen. Jos. G. Totten, U. S. A.
Col. J. J. Abert, U. S. A.
Lieut. M. F. Maury, U. S. N.
Lieut. C. H. Davis, U. S. N.
Peter Force,
Prof. A. Guyot,
Lieut. E. B. Hunt, U. S. A.

Committee to Memorialize Congress for an Appropriation to enable Professor Mitchel *to perfect and apply his new Astronomical Apparatus.*

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Lieut. M. F. Maury, U. S. N.
Prof. Joseph Henry.

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Prof. J. Lawrence Smith, *General Secretary.*
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Prof. S. F. Baird,
Dr. A. L. Elwyn.

Local Committee.

Hon. J. W. Maury, *Chairman.*
Prof. J. H. C. Coffin, *Secretary.*

MEETINGS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

	Place.	Date.	President.	General Secretary.	Permanent Sec'y.	Treasurer.
1st Meeting,	Philadelphia, Pa.	September 20, 1848,	W. C. Redfield, Esq.,	Prof. Walter R. Johnson,	. . .	Prof. J. Wyman.
2d "	Cambridge, Mass.	August 14, 1849,	Prof. Joseph Henry,	Prof. E. N. Horsford,	. . .	Dr. A. L. Elwyn.
3d "	Charleston, S. C.	March 12, 1850,	Prof. A. D. Bache,*	Prof. L. R. Gibbes,*	. . .	Dr. St. J. Ravenel.*
4th "	New Haven, Ct.	August 19, 1850,	Prof. A. D. Bache,	E. C. Herrick, Esq.	. . .	Dr. A. L. Elwyn.
5th "	Cincinnati, Ohio,	May 5, 1851,	Prof. A. D. Bache,	Prof. W. B. Rogers,	Prof. S. F. Baird,	Dr. A. L. Elwyn.
6th "	Albany, N. Y.	August 19, 1851,	Prof. L. Agassiz,	Prof. W. B. Rogers,	Prof. S. F. Baird,	Dr. A. L. Elwyn.
7th "	Cleveland, Ohio,	July 28, 1853,	Prof. Benj. Peirce,	Prof. J. D. Dana,†	Prof. S. F. Baird,	Dr. A. L. Elwyn.

* In the absence of the regular officer.

† In the absence of Professor Dana, Professor S. St. John discharged the duties of General Secretary.

MEMBERS ELECTED AT THE CLEVELAND MEETING.

Abbott, Gorham D., New York.
Adamson, Rev. Wm., Cambridge, Mass.
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Warner, Edward, New Brighton, Beaver Co., Penn.
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Williams, Prof. George P., Ann Arbor, Mich.
Williams, Mat. J., Columbia, S. C.
Wilson, Prof. John, London, Eng.
Wilson, John, Staunton, Va.

Wood, Prof. Alphonso, Cincinnati, Ohio.
Woodbury, Peter T., New York.
Woodrow, Prof. James, Milledgeville, Geo.
Woodruff, Luni, Ann Arbor, Mich.
Wormley, Theo. G. Columbus, Ohio.
Wright, Albert B., Perrysburg, Ohio.
Wright, J. J. B., M. D., U. S. A., Carlisle Barracks, Penn.
Young, Prof. Ira, Hanover, N. H.
Young, J. A., M. D., Camden, S. C.

NOTE.

As the full list of members has just been printed in the Providence volume, it was thought unnecessary to reprint it in this volume. But the name of B. FRANKLIN GREENE, of Troy, N. Y., should be added to that list.

OPENING ADDRESS,

BY

PROFESSOR BENJAMIN PEIRCE,

OF CAMBRIDGE,

PRESIDENT OF THE ASSOCIATION.

Gentlemen of the American Association for the Advancement of Science: —

We are again met in the service of our high cause; after the unusual interval of two years, we have again come together at our appointed rendezvous, to make each other glad with the tidings of truth which we bring from the heavens and the earth, and to reanimate our fainting zeal by the story of the successful search for the philosopher's stone, the true *elixir vitæ*, the fruit of the tree of knowledge, and the footprint of Him of whom the earth is the footstool.

Gentlemen, from such an assembly egotism shrinks abashed, and you will not reprove your President that he does not intrude his feelings of grateful pride at the honor which you have conferred upon him, and his profound sense of his incapacity to wear the robes of Redfield, and Henry, and Bache, and Agassiz. His hopes of success in the discharge of his duties do not arise from the vigor of his own energy or the readiness of his own wisdom; but from the manly hearts which surround him on all sides, — hearty friends whose generous sympathy will easily forgive and correct the errors of an honest purpose.

Gentlemen, we are not convened for a light duty, — our

self-imposed task is not an amusing child's play; and we have not accepted the liberally offered hospitalities of this beautiful city for the enjoyment of a social festival. We have come to give and receive instruction and inspiration. We have come to the shores of this great lake to admire and study the pebbles which our brethren may have picked up here and there with much labor, and to learn where or how they are to be found. We have brought our freights of knowledge to distribute them to the world, that they may do good.

Gentlemen, we have come to study our duty as scientific men, and especially as American scientific men. We are to learn the apparent, and not very pleasant paradox, that America cannot keep pace with Europe in Science except by going ahead of her. The New World must begin to build upon a level above that of the Old World, and it must build from its own materials. This is not asking too much. It is no more than was accomplished by the American Ship and the American Reaping-Machine. The Yankee who picked the hardest lock of England, and contrived a lock which all England could not pick, is but a type of American intellect. This was a work of mind, and we have a right to expect equal excellence in the higher and more abstract efforts of American genius. But, above all things, it is not to be forgotten that the Temple of Science, by whomsoever built, belongs to no country or clime. It is the World's Temple, and all men are free of its communion. Let us not mar its beauty by writing our names upon its walls. The stone which we have inserted is not ours,—it is not thine, it is not mine,—but it is part of the Temple. The child picks up a shell, innocently admires its form and coloring, and listens, without a thought of self, to the singing of the angels within it. It is the unconsciousness of the attitude which gives it its grace and beauty, and makes the child and the shell part of the same divine thought, each for ever belonging to the other, and both immortalized in the marble of the artist.

Gentlemen, let us stand here reverently. This is holy ground. Let us not presume to make these walls resound with the bickerings of angry contention for superior distinction, and the foul complaints of mortified vanity. Let us not raise the money-changer's cry of mine and thine, lest the purifier come, and, taking the royal jewel into his own possession, thrust us out into the ditch, and turn our fame into infamy.

It has been observed by others, not of our number, that the meetings of the Association have been characterized by a generous appreciation of each other's labors, and it has naturally contributed to the influence and power of our Society. May we continue this honorable harmony, so fitting to our sublime studies, and be always open to the reception of new discoveries and new discoverers. But mutual admiration is not our only or our most necessary office. Mutual criticism is equally conducive to the best interests of the Association. We should exert ourselves to restrain vagueness and uncertainty of thought and expression, and to prevent the concealment of old truths under new forms. We must not permit erroneous statements to pass unchallenged. It is our stern and solemn duty to criticise and expose all false developments, whether they are intended, or the unintentional results of carelessness or ignorance. The task may and should be performed with delicacy and generosity, and the mode of performance will clearly manifest the spirit of the operator, and mainly determine the success of the operation. The knife, wielded with unsparing rudeness, is less effective than the touch of Ithuriel's spear.

> "For no falsehood can endure
> Touch of celestial temper; but returned
> Of force to its own likeness; up it starts
> Discovered and exposed."

Gentlemen, let us learn wisdom from the poet.

There is another point to which I must allude before closing these opening remarks, which are already too much pro-

tracted. We have a constitution and laws, and it is our duty to observe them. If in any important respect we have deviated from our fundamental laws, we should either retrace our steps or amend our constitution. But let us not forget that unskilled lawyers may easily become pedants of law, and observe that this is an Association of gentlemen whose garments are intended for easy and graceful protection, and not a Bedlam, whose denizens are to be strictly confined within strait waistcoats.

And now, Gentlemen, without further delay, let us proceed to the business for which we have assembled.*

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* The usual address delivered by the retiring President was not given this year, on account of the absence of Professor Louis Agassiz.

PROCEEDINGS

OF THE

CLEVELAND MEETING, 1853.

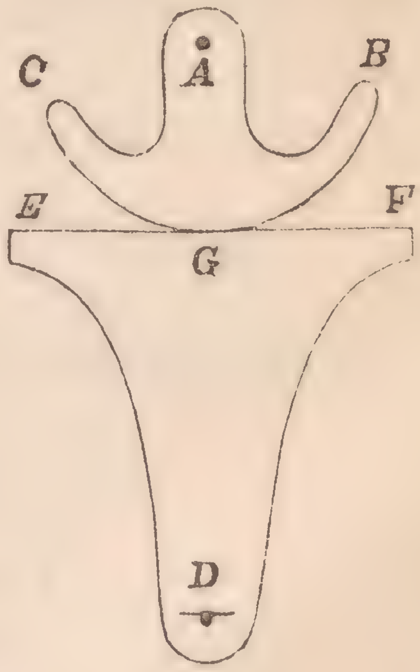
COMMUNICATIONS.

A. MATHEMATICS, ASTRONOMY, AND PHYSICS.

I. MATHEMATICS.

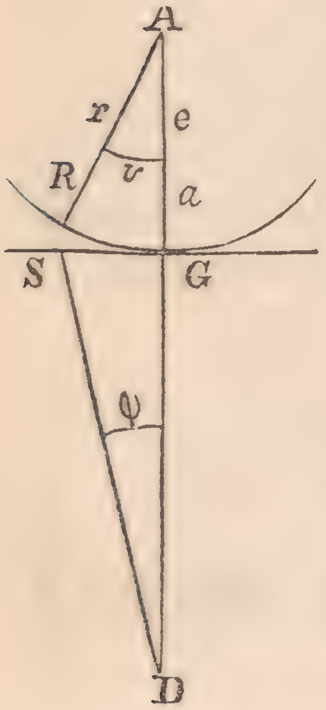
1. Mathematical Analysis of the Contact of Surfaces in Oscillating Machinery. By Professor C. W. Hackley, of New York.

The subject of this paper was suggested by a recently invented printing-press, of which the diagram represents a vertical section, and in which the following movement was required: the upper portion, of which $A\ C\ B$ is the section, was to revolve about an axis perpendicular to the plane of the section at the point A, whilst the lower portion, of which $E\ D\ F$ is the section, revolved about an axis passing perpendicularly to the plane of the section through the point D; the plane surface, or type-bed, of which the straight line $E\ G\ F$ is a section, and the curve surface, of which $C\ G\ B$ is a section, were required to be in contact throughout the whole movement without sliding upon each other.



The mathematical problem involved in this arrangement is the de-

termination of the differential equation of the proper curve, $C\ G\ B$, the radius of its osculatory circle at the point G, and the degree of coincidence of the circumference of the latter with the curve, to an extent sufficient to cover the type, in order to the use of the circular form if possible.



Let the variable arc $G\ R$ be denoted by z, the radius vector $A\ R$ by r, the variable angle, $G\ D\ S$ by φ, varying in such a manner in relation to v, that $G\ R$ and $G\ S$ shall be always equal. Let the constant distance between the centres A and D be denoted by a, and the constant distance $D\ G$ by c.

By the conditions of the problem, $G\ R$ and $G\ S$ must increase at the same rate, $A\ R$ at the same time decreasing at the same rate that $D\ S$ increases, so that $D\ S + A\ R$ may be constant and equal to $D\ G + G\ A = a$.

The first two conditions are expressed by the two differential equations,

$$dz = d\ (c \tan \varphi) \ldots . (1.)$$

$$dr = -d\ (c \sec \varphi) \ldots .. (2.)$$

Substituting for the first member of (1) its value in polar co-ordinates, and for the second members of (1) and (2) their equivalents obtained by effecting the differentiation, these equations become, after squaring,

$$dr^2 + r^2\,dv^2 = c^2 \sec^4 \varphi\, d\varphi^2 \ \ldots . (3.)$$

$$dr^2 = c^2 \tan^2 \varphi \sec^2 \varphi\, d\varphi^2 \therefore d\varphi^2 = \frac{dr^2}{c^2 \tan^2 \varphi \sec^2 \varphi}.$$

By substitution of this value of $d\varphi^2$ in (3),

$$dr^2 + r^2\,dv^2 = \frac{dr^2}{\sin^2 \varphi}.$$

Or, $$(1 - \sin^2 \varphi)\, dr^2 = \sin^2 \varphi\, r^2\, dv^2.$$

Or, $$\cos^2 \varphi\, dr^2 = \sin^2 \varphi\, r^2\, dv^2.$$

Or, $$\cos \varphi\, dr = -\sin \varphi\, r\, dv^* \ \ldots . (4.)$$

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* The second member is negative, because r is a decreasing function of v.

But from the diagram,

$$\cos \varphi = \frac{c}{a-r} \quad \therefore \sin \varphi = \sqrt{1 - \frac{c^2}{(a-r)^2}}.$$

By substitution of these values, (4) becomes

$$d\,r = -\frac{r}{c}\sqrt{(a-r)^2 - c^2}\,d\,v \quad . \; . \; . \; . \quad (5),$$

which is the differential polar equation of the required curve, the pole being at A, and the fixed line of the polar co-ordinates, $A\,G$.

To determine the radius of curvature, differentiate (5),

$$\frac{d^2 r}{d\,v} = -\frac{1}{c}\left(\sqrt{(a-r)^2 - c^2} - \frac{(a-r)\,r}{\sqrt{(a-r)^2 - c^2}}\right) d\,r.$$

Substituting the value of $d\,r$ given by (5), this becomes

$$\frac{d^2 r}{d\,v^2} = \frac{r}{c^2}\left[(a-r)^2 - c^2 - (a-r)\,r\right] \; . \; . \; . \; . \; (6.)$$

Substituting these values of the first and second differential co-efficients in the general formula, for the radius of curvature in polar co-ordinates,

$$\rho = \frac{\left(r^2 + \frac{d\,r^2}{d\,v^2}\right)^{\frac{3}{2}}}{r^2 - r\frac{d^2 r}{d\,v^2} + 2\frac{d\,r^2}{d\,v^2}},$$

it becomes

$$\rho = \frac{\left\{r^2 + \frac{r^2}{c^2}\left((a-r)^2 - c^2\right)\right\}^{\frac{3}{2}}}{r^2 - \frac{r^2}{c^2}\left((a-r)^2 - c^2 - (a-r)\,r\right) + 2\frac{r^2}{c^2}\left((a-r)^2 - c^2\right)},$$

which is the expression for the length of the radius of curvature at any point of the curve $G\,R$, in terms of the radius vector at the point. At the point G, for which

$$r = a - c \;\therefore\; a - r = c,$$

it becomes

$$\rho = \frac{((a-c)^2)^{\frac{3}{2}}}{(a-c)^2 + \frac{(a-c)^2}{c^2}(a-c)\,c},$$

which readily reduces to

$$\rho = \frac{c}{a}(a-c) \quad . \; . \; . \; . \; . \quad (8.)$$

That is, the radius of curvature at G is the same fraction of the radius vector at that point, $A\ G$, that the distance of the lower centre of oscillation, D, from that point, or $D\ G$, is of the distance between the centres of oscillation, or $A\ D$.

If a be taken 60 inches, and c 45, the radius of curvature by (8) will be 11.25 inches. If, now, 10 inches * of arc on each side of the point c be computed to this radius, taking the radius equal in length to an arc of 57°.2957795, its known value, the arc of 10 inches, or the angle which it measures, will be found to be 50° 55′ 46″. With the supplement of this angle, and the two containing sides, 11.25 and (60 — 45) — 11.25, or 3.75, the length of $A\ R$ being computed; also with $G\ S = 10$, the length of $D\ S$ being computed; these lines will be found to be 13.9213 and 46.0980, and their sum, 60.0193. That is, within less than $\frac{1}{50}$ of an inch of what it would be if the true curve, instead of the arc of the osculatory circle, were employed.

To find now the equation of the curve, let (5) be resolved with respect to $d\,v$, and integrated.

$$-d\,v = \frac{c\,d\,r}{r\sqrt{(a-r)^2 - c^2}};$$

$$\therefore -v = c\int \frac{d\,r}{r\sqrt{(a-r)^2-c^2}}.$$

Effecting the integration by the rules for irrational trinomials of the second degree, and for partial fractions, determining the arbitrary constant by observing that when $v = 0$, $r = a - c$, $\therefore a - r = c$, $\therefore (a-r)^2 - c^2 = 0$, there results

$$v = \frac{c}{\sqrt{a^2-c^2}} \log \left\{ \frac{[-(a-c) - \sqrt{a^2-c^2}]\,[\sqrt{(a-r)^2-c^2} - r + \sqrt{a^2-c^2}]}{[-(a-c) + \sqrt{a^2-c^2}]\,[\sqrt{(a-r)^2-c^2} - r - \sqrt{a^2-c^2}]} \right.$$

as the polar equation of the required curve, which is transcendental.

Substituting the values before used, viz. $a = 60$, $c = 45$, $r = 13.9$, v will be found to be 0.75 of the radius unity, or about 43°, which it should be, and this verifies the correctness of the equation.

To generalize the problem, that is, if two cylindric surfaces be supposed to revolve in contact, without sliding, about two axes parallel to their elements and to each other, the transverse section of one cylinder being any given curve, to find that of the other.

* This would be about the dimensions for a press of the ordinary size.

Let $$r = Fv$$
be the polar equation of the required section,

$$\rho = f\varphi \therefore \varphi = f^{-1}\rho$$

that of the given. Then, by the condition of the problem,

$$r + \rho = a \therefore \rho = a - r$$

$$dr^2 + r^2 dv^2 = d\rho + \rho^2 d\varphi^2;$$

or, since $d\rho^2 = dr$,

$$r^2 dv^2 = \rho^2 d\varphi^2;$$

$$\therefore r^2 dv^2 = (a-r)^2 [df^{-1}(a-r)]^2 \ldots (7),$$

which is the differential equation of the required curve, and depends evidently on the form of the equation of the given curve.

To apply this general form to the particular case of the right-lined section already considered for the purpose of verification, assume the polar equation of the right line,

$$\rho = c \sec\varphi \therefore \varphi = \sec^{-1}\frac{\rho}{c}$$

and (7) becomes for this,

$$r^2 dv^2 = (a-r)^2 \left[d \sec^{-1}\frac{a-r}{c}\right]^2,$$

$$\therefore r^2 dv^2 = \frac{c^2 (a-r)^2 dr^2}{(a-r)^2 [(a-r)^2 - c^2]},$$

or,

$$\therefore r^2 dv^2 = \frac{c^2 dr^2}{(a-r)^2 - c^2},$$

which is identical with (5).

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2. On the Method of finding the Error of a Chronometer by Equal Altitudes of the Sun. By Professor W. Chauvenet, of the U. S. Naval Academy.

The method of finding the error of a chronometer by equal altitudes of the sun, taken in the morning and afternoon, is considered to be the most accurate available to the navigator. As commonly taught, however, in works of navigation, there exist some small inaccuracies, which it is desirable to avoid. The equation of equal alti-

tudes, or the correction of the mean of the two chronometer times, to reduce it to the chronometer time of the sun's transit, is to be computed by the known formula,

$$\text{Correction for noon} = -\frac{\Delta\,\delta \tan\phi}{15 \sin\frac{1}{2}E} + \frac{\Delta\,\delta \tan\delta}{15 \tan\frac{1}{2}E};$$

in which E = elapsed time between the A. M. and P. M. observations,
ϕ = latitude of the place of observation,
δ = sun's declination at noon,
$\Delta\,\delta$ = change of declination from noon to either observation.

Now $\Delta\,\delta$ is not precisely the same for each observation, if we consider the second differences of the declination; and accordingly Gauss (who first put the correction under the above form) uses the 48 hour change of declination from the noon preceding to the noon following that of the observation. This very nearly allows for second differences, and, when the logarithm of the 48 hour change is given in the ephemeris, as in the *Berlin Jahrbuch*, is a sufficiently convenient method. But when this logarithm is not given, most navigators find it inconvenient to deduce the change from the declinations themselves. The method becomes at once most accurate, and, at the same time, most simple, by deducing $\Delta\,\delta$ from the hourly change of the sun's declination at the instant of apparent noon, as this evidently allows for second differences; but as the *British Nautical Almanac* is arranged, this is still inconvenient; because the hourly differences there given are nothing more than the differences between the declinations at two successive noons, divided by 24, and therefore do not correspond to noon, but nearly (though not exactly) to midnight. In the *American Ephemeris*, the proper hourly difference is given, namely, that which belongs to the instant of noon; and, in order to make this improvement at once available to the navigator, I have prepared a new table for computing the equation of equal altitudes, adapted to the new arrangement of the ephemeris. I give in the table, with the argument "elapsed time," the logarithms of

$$A = \frac{E}{1800 \sin\frac{1}{2}E}, \qquad B = \frac{E}{1800 \tan\frac{1}{2}E},$$

in which E in the numerator is expressed in minutes of time, and the above formula is reduced to

$$\text{Corr. for noon} = -A\,.\,\Delta_0\,\delta \tan\phi + B\,.\,\Delta_0\,\delta \tan\delta,$$

in which $\Delta_0\,\delta$ is the hourly difference of declination at apparent noon.

The argument is extended from 0 to 24 hours, in order that the table may serve for computing the correction for midnight, in the case where one altitude is taken in the afternoon, and the corresponding equal altitude on the following morning, the formula for this case being,

$$\text{Correction for midnight} = A \,.\, \Delta_0\, \delta \,.\, \tan \phi + B \,.\, \Delta_0\, \delta \,.\, \tan \delta,$$

in which $\Delta_0\, \delta$ is the hourly difference of declination for midnight, and A and B are the same functions of the elapsed time as already given.

It may not be improper for me here to point out some errors that exist in standard works designed for the use of the navigator.

In Lieutenant Raper's *Practice of Navigation*, a work of high authority, the correction for midnight is incorrectly computed. His error is similar to one we should commit if we were to substitute $12^{\text{h.}} - E$ for E in the above formulas for A and B, an error which completely vitiates the whole method.

A small error exists in the rule laid down in Professor Inman's Navigation, where the equation of time to be applied to the chronometer time of transit, in finding the chronometer time of mean noon, is taken for mean instead of apparent noon. The change of declination is not found by his rule with the greatest accuracy for a similar reason.

In Galbraith's *Tables, &c.*, a somewhat tedious rule is laid down for both the noon and midnight corrections, which is correct as far as it goes, but incomplete in not providing for all the cases. In the A. M. — P. M. observation, or that in which the correction for noon is required, we *may* have an elapsed time greater than 12 hours; while in the P. M. — A. M. observation, we *may* have an elapsed time less than 12 hours, and neither of these cases is met by Galbraith's rule.

It is important to notice such imperfections in works designed for practical men, who are not always able thoroughly to investigate the rules themselves, and who are embarrassed by inconsistencies in their results without being able to trace their causes. In such cases they are very apt to abandon a good method altogether, and adhere to some less accurate one, which, at least, never leads them into difficulty.

The tables I have prepared are designed to form a part of the Appendix to the *American Ephemeris*, and will be accompanied with the necessary rules for the practical navigator.

II. PHYSICS.

1. On Cohesion of Fluids, Evaporation, and Steam-Boiler Explosions. By Lieutenant E. B. Hunt, of Newport.

I now wish to present a simple exposition of the mechanical theory of cohesion in fluid masses, and from this to deduce the structure of a fluid surface, showing that its cohesive strength is much less than that of the interior layers. This result furnishes a clear and direct explanation of the great fact of evaporation, and shows why, in all cases, even in ebullition, evaporation is a strictly surface phenomenon. Hence follows an explanation of one of the chief causes of steam-boiler explosions, and the easy suggestion of a very practical remedy; also, an explanation of the heating of fluids to high temperature as observed by Donny, and of the entire agency of contained air in ebullition.

Several years have now elapsed since, in tracing out the results of a highly general theory of molecular mechanics, it occurred to me to call in question the commonly received views as to the amount and character of fluid cohesion. Regarding all cohesions as directly a function of the distance between adjacent molecules, it was quite impossible to imagine that the exceedingly small difference of the intermolecular distances corresponding to the fluid and solid forms respectively in any given substance, could produce that very great difference of cohesive strength so generally conceived to exist. The slight difference of volume, for instance, between a solid and fluid pound of iron, would not lead us to anticipate any marked difference of cohesion, so long as we regard this cohesion as any tolerably simple function of the intermolecular distance.

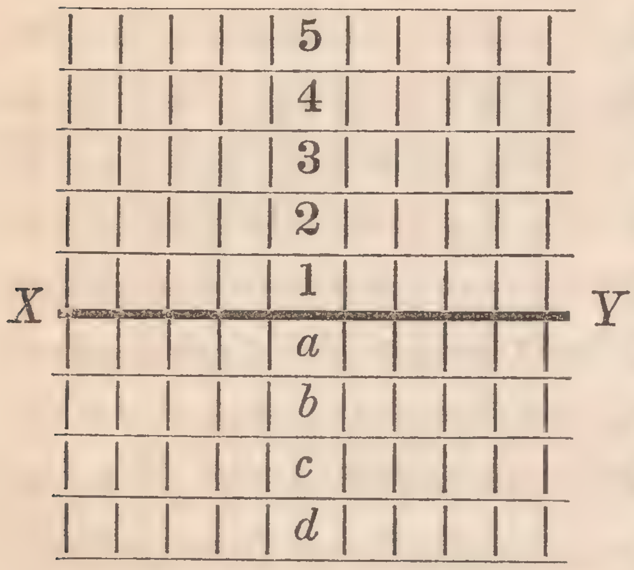
The ordinary experiments professing to measure fluid cohesion are by no means cases of direct rupture, and, indeed, furnish no measure whatever of actual cohesive strength. The common experi-

ment of separating, by counterpoising weights, a disk from a fluid which wets it, furnishes no indication of the cohesion in the mass of fluid, but merely shows the force required to break the fluid surface. Donny's experiments show positively that the yielding is here entirely at the surface, progressing through the mass by the successive breaking of the successively formed surfaces, only a mere fluid filament being at last broken by direct rupture. It is truly a case of capillary action between a horizontal fluid surface and a horizontal circular solid surface, and, like all other capillary action, exists primarily at the surface only. Except in the frequently observed adhesion of well-boiled mercury in barometer-tubes to heights far above the true barometric level, we have, in fact, no record of any experiments exhibiting the resistance offered by a fluid mass to direct rupture, which only ought to be taken as a true measure of cohesion. All the common views of a slight fluid cohesion are based on erroneous interpretations, in which the effects of the easy mobility of parts in fluids are very loosely imputed to a low value of cohesion. Once clearly understanding that surface-yielding gives no measure of cohesion or direct resistance to rupture, we can readily see that the prevalent ideas on this subject are without support.

If we study the phenomena attending the condensation of gases and vapors into fluids, it is apparent that, while contiguous molecules are still at distances many times as great as that characterizing the fluid state, the cohesive attraction manifests itself appreciably, steam instantly condensing at the rate of a foot of steam to an inch of water, showing that in water the cohesive action of a molecule extends effectively through a sphere whose diameter is at least twelve times the distance between adjacent molecular centres in the fluid. Hence, in water, the radius of the effective cohesive action must be so great as to include several molecular layers. The moment a gas ceases to follow Marriotte's law, cohesive action becomes appreciable, and this is proof enough that in masses many layers contribute their action in making up the total cohesion.

If we conceive any fluid mass to be distributed into layers, then the correct measure of fluid cohesion will be the force requisite to produce a direct, simultaneous separation of all the parts along a unit of the dividing surface between two layers. This is equal to the resultant of all the forces acting from either direction against this unit of sur-

face, these forces being held *in equilibrio* by the equal opposing forces. To obtain an expression for this cohesion, let the fluid mass be conceived as divided into elementary layers one molecule in thickness relative to three perpendicular co-ordinate axes. Let the layers above the plane $X\,Y$ be called 1, 2, 3, &c., those below being called a, b, c, &c.



Take the unit of surface in the plane $X\,Y$ between layers 1 and a. Then the force with which the unit in layer 1 presses against layer a is composed of all the attractions which the entire layers a, b, c, &c., exert on the units in layers 1, 2, 3, &c., which make up the prism basing on the unit of surface. Or, calling the cohesion x, and designating the elementary forces by the layers between which they are exerted,

we have

$$\left.\begin{aligned} x = a, 1 + b, 1 + c, 1 + d, 1 +, \&c. \\ + a, 2 + b, 2 + c, 2 +, \&c. \\ + a, 3 + b, 3 +, \&c. \\ + a, 4 +, \&c. \end{aligned}\right\}$$

in which the terms arranged above each other have equal values. This series would require to be extended so as to include all terms corresponding to distances at which cohesive force may not be regarded as evanescent. By assuming some law of connection between this force and the distance, an integration of effect could be attained; but this is not now necessary. An inspection of the formula gives the main features in the mechanism of cohesion within masses, either solid or fluid.

In order now to study the peculiarities of constitution belonging to surfaces, let us, in this formula, introduce the condition that layer 1 shall be a surface layer. All terms containing 2, 3, 4, &c. are thus stricken out, and we have as the surface cohesion along the normal direction,

$$x = a, 1 + b, 1 + c, 1 + d, 1 +, \&c.$$

But in the general expression, we have, by observing the equality of terms,

$$x = a, 1 + 2\,(b\,.\,1) + 3\,(c\,.\,1) + 4\,(d\,.\,1), +, \&c.$$

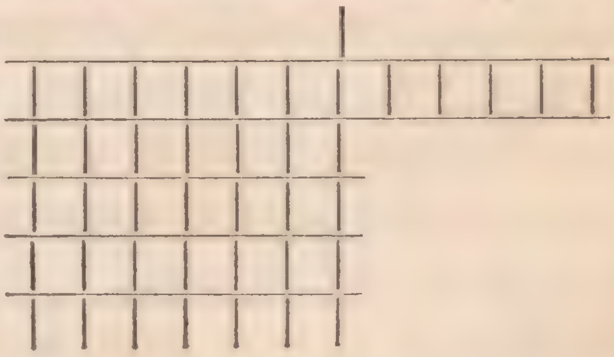
Comparing these values of x, we see that the surface layer coheres to the mass with a very much smaller force than two internal layers cohere against each other. For the second, third, &c. layers, a like discussion applies, and the cohesion gradually increases in penetrating the mass.

This formula involves no particular hypothesis as to the value or character of the forces acting, only that the aggregate is attractive. But as condensation is a spontaneous phenomenon through all that portion of aggregational range in which energetic actions are found, we ought to assume that all the effective terms are attractive. To present the grounds which seem to me to authorize the conception that repulsion in all states of aggregation is only exercised between adjacent molecules, while the attractive actions are the resultants of all the primary constitutional forces, and extend through larger spheres, would involve the exposition of a complete theory of molecular mechanics.

I must therefore leave as an assumption, the conception that, in fluids, the only repulsion to be taken into account is that of the contiguous layers, which prevents their yielding farther to the cohesive forces pressing them together.

We should observe that, in consequence of the deficiency of cohesion along the fluid surface, a rarefaction would take place, which would again diminish surface cohesion to a considerable extent below that value given by the formula.

To determine the cohesion measured along a surface, as we have done for that along the normal, let the general formula be applied to a surface element. Then, instead of the normal layers being full layers, they are essentially but half-layers, or each term has approximately only one half of its value for the interior. Hence, the value of x is approximately only one half of its interior value, or the cohesion along a surface is about one half what it is within the mass. But as this value gives a rarefaction also along the surface, as well as along the normal, it will therefore be much diminished, so as to become less than one half the general value. Thus, both along the normal and along the surface, a weak cohesion is a necessary characteristic of the bounding layers



of material masses, both fluid and solid. The result thus reached in respect to a mass *in vacuo*, would not be greatly affected in the ordinary atmosphere.

It is somewhat remarkable that Poisson's capillary theory, as stated by Mossotti, in Taylor's *Scientific Memoirs*, is based essentially on an analysis of the fluid surface, in which the halfing of the normal layer is totally overlooked, and the cohesion along the surface is said to be the same as in the mass, the surface layer only having been taken into account. I have not seen Poisson's work, but it is singular that Mossotti should either have made such an oversight, or have failed to detect it in Poisson, if he really committed it. It is a radical defect, even using Poisson's own hypotheses, and must directly affect or invalidate his whole theory.

I come now to an important deduction from the preceding discussion. Fluid surfaces are in a state of weak cohesion, as compared with fluid interiors ; hence a partially atmospheric condition of rarefaction exists along such bounding surfaces. If, then, we assimilate heat to a molecular repulsion, as is customary, we see at once that, as the temperature is raised, the weak cohesion in the surface will be overcome long before the mass is heated to that point which will overmaster its internal cohesion. Hence the surface molecules will freely pass off as vapor, while a strong cohesion still exists throughout the entire mass. Evaporation thus goes on at the surface at all temperatures above that which just suffices to overcome the weak surface cohesion. This constitution or structure, necessarily characterizing the limiting layers of fluids, is the true and full explanation of evaporation in all its forms. From this we see that a fluid mass without interior or exterior surfaces, or so enclosed as virtually to answer this description, might be heated up far above the boiling point without boiling. We see that ebullition is but the effect of an internal evaporation, starting in minute air-bubbles, and growing with the expanding bubble. We see that water entirely freed from air-bubbles, and with a restricted open surface, as in Donny's late experiments, should go on heating up far above the boiling point, until at last the whole heated mass would flash into steam with an explosion. All the phenomena described by Donny in his excellent paper in the *Annales de Chimie et de Physique*, follow, as easy and obvious deductions, from this constitution of the fluid surface.

Indeed, we do not at all wonder at his being forced, from his experiments, to conclude empirically that there must be some peculiar quality in surfaces, which makes evaporation take place so much more readily on them than in fluid masses. We see, too, how utterly fallacious are the experiments usually regarded as measuring fluid cohesion, since they only deal with the weak cohesion in surface layers, which, with the free mobility of fluid parts, fully explains all the observed results. This view explains how a too perfect boiling of the mercury in barometer-tubes makes it adhere at the top with such tenacity. It explains Berthollet's experience on the forced dilatation of fluids, in which a deaerated fluid, sealed when hot, does not shrink in cooling for a long time, but at last breaks and collapses, indicating that it has borne a great tension before yielding. Professor Henry's elegant experiments with soap-bubbles, in which, by measuring the tension of the enclosed air, he is able to deduce, first the compressing force, and then the cohesion of the fluid film, furnish an independent confirmation of the same general views. We may remark, that the heterogeneous structure of the outer layers would destroy the mobility of their parts, and give a film-like character to the fluid surface, while all within this film would have free mobility. This, with the additional fact of a drawing inward of the outward layers by the unbalanced cohesive action of the layers near the surface, explains the great variety of formal phenomena exhibited by drops, bubbles, and fluid surfaces.

About four years since, I conceived the idea of directly measuring fluid cohesion, by rupturing a pure fluid column in a cylinder with a moving piston. By filling the cylinder with the fluid to be tested, and immersing the piston by the aid of a valve closing at will, the force requisite for starting the piston will be the cohesion of the column after allowing for atmospheric pressure. Of course the fluid must adhere to the cylinder more strongly than it coheres in itself, else the adhesion only would be measured. Nor must it contain any air-bubbles, as the presence of one such, however small, will give a start to the break, by presenting a weak surface. This is the great difficulty of the proposed experiment. In May last, I had just begun such an experiment on mercury in an amalgamated cylinder; but the requisite precaution for excluding the air could not be taken, for lack of time, as I was obliged to leave my station before the apparatus was com-

pleted. The rapidity with which the mercury rushed past the piston in the rough trial made, showed that some packing will probably be requisite to a deliberate measurement, and this again will present the difficulty of introducing an unamalgamated surface in the mass to be broken. The precautions requisite for a perfect trial of the experiment are quite numerous.

I anticipate that exceedingly small air-bubbles will have the effect of making the indications irregular, as the smallest bubbles will only start a break on the application of a very considerable force.

I will now apply this discussion to *steam-boiler explosions.* The condition requisite for ebullition in boiling water is simply that air-bubbles, in the heated portions, shall present on their borders the weakly coherent surfaces requisite for evaporation to be established. Perfectly deaerated water, with a limited surface, would not boil at all, but would steadily heat up until it reached that point at which it would flash explosively into steam. Now one chief cause of local explosions is clearly of this description. The boat stops at a wharf; the "doctor," or pump supplying water to the engine, being worked by the engine itself, stops its water supply when the engine stops. The water in the boiler goes on boiling until all the air-bubbles are boiled off from the water, and their air is mixed with the steam above. Thus there ceases to be any evaporating surface except that on the top layer, which is farthest from the heated surface, and quite inadequate to the consumption of all the heat supplied. Then the mass of water begins to heat up, and it goes on storing up the unconsumed caloric, until the water is far hotter than the head of steam would indicate. The engineer starts the engine, this starts the pump, which throws a stream of water, charged with air, directly into the glowing fluid. The heat instantly finds its outlet by an overwhelming evaporation on the newly supplied bubble surfaces, and a tumultuous ebullition follows. The gathered store of heat flashes off a portion of the water into steam of excessive tension, — a tension such as nothing can withstand. The terrific consequences are too often witnessed in those fatal catastrophes which have given to our Western rivers such a tragic reputation.

No one can examine a list of Western steamboat explosions, without being forcibly impressed with the frequency of these accidents just as the boat is starting from the wharf after landing. It seems to me

beyond doubt, that many of these occur just in the manner now stated, and from the deficiency of air-bubbles in the boiler. We see in this reasoning, too, a sufficient explanation of dry steam, or steam hotter than its tension indicates. The heating is then going on faster than the evaporation, and the steam is thus heated as if it were not in contact with the water, or were in a vessel by itself.

It is not always that a remedy for an error is as obvious and as easily applied as in this case. It is only necessary to keep the pump in steady, slow operation, while the engine is at rest. It should always be capable of an independent movement, and should constantly, while a boat is fired up, be kept at work, however slowly. By this means, air for ebullition will always be supplied, and the accumulation of heat in a sluggish mass of water cannot then go on until the explosive point is reached.

The field over which I have thus rapidly traversed, is one requiring much practical study for its full development and illustration. I could not have given all which belongs to it without exceeding reasonable limits. Nearly all the views which I have presented were the result of my own studies, so far as concerned my original acquaintance with them. I was happy to find that Donny and Henry had, in some points, reached the same conclusions by independent routes ; but I am not aware that any one has presented the same analysis of cohesion, or of the molecular constitution of material surfaces. Especially does the derivation of evaporation from molecular mechanics seem to me novel, and worthy of careful consideration. Donny indicates deaeration as a cause of steam-boiler explosion, but it is essentially an experimental deduction, and not connected with its mechanical derivation.

In conclusion, I will present an outline of a most interesting illustration of creative design in the earth's co-ordination. The explanation of evaporation which has been given, shows that for each fluid the formation of vapor lies within certain definite limits of temperature, as a result of its primary structure. These limits differ greatly in different fluids. Now, in framing the earth for habitation, or for the proper life of animal and vegetable forms, something equivalent to rain was necessary, from the constant descent of fluids to the lowest level. Without some agency to lift the great organic fluid above the lowest ocean-bed, sterility would have been the lot of all which rose above its surface, and terrestrial organisms would have been quite impossible. But fluidity does not involve evaporation,

except within certain definite limits, special for each liquid. Again, evaporation might freely go on, and yet no capacity for condensation exist, except within other limits of temperature quite unattainable, save through special arrangement. Rain, then, with our earth and atmosphere, involved a special constitution of the raining fluid, not only so that evaporation at ordinary temperatures should go on, but so that condensation may again take place in the ordinary air. Not only must this qualitative arrangement exist, but also a quantitative one, since the quantity of rain best sufficing to the aggregate organic need is exactly a certain definite number of inches per annum.

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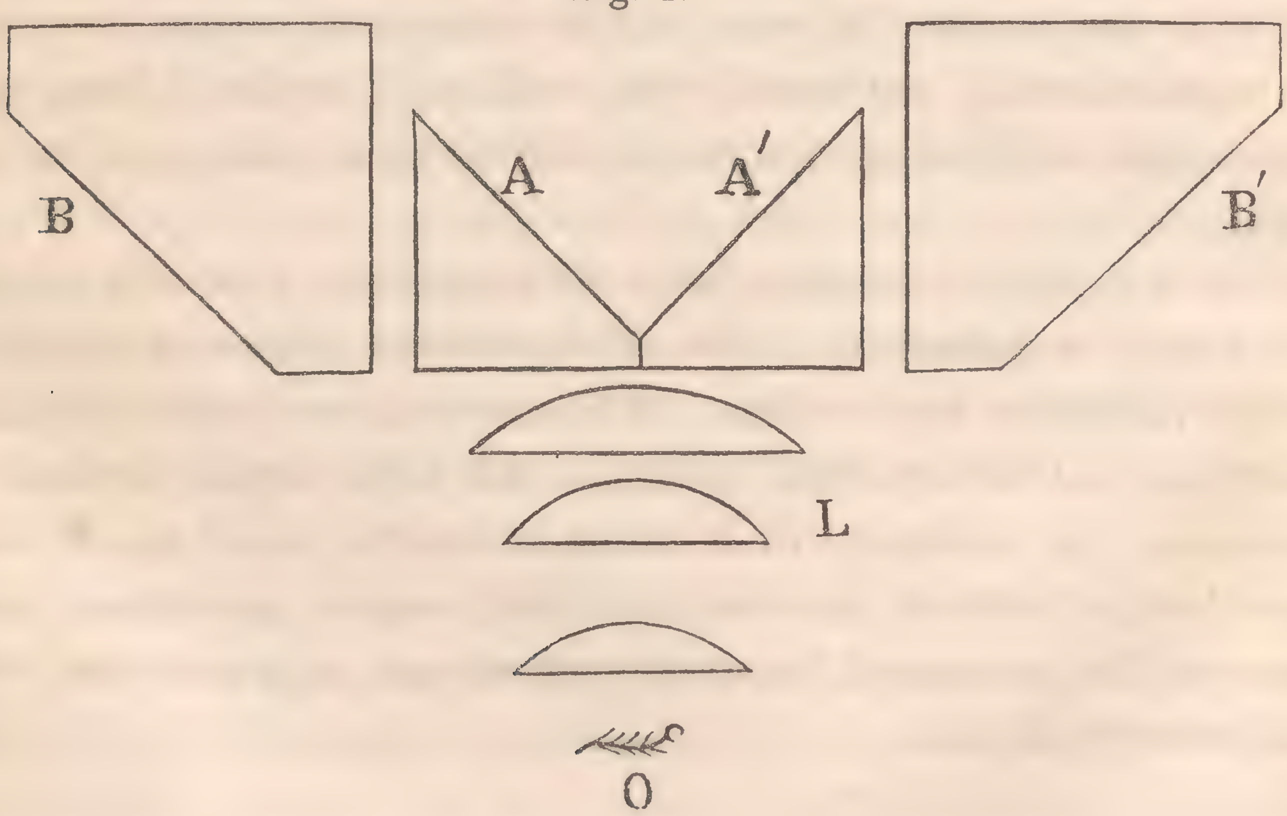
2. On the Binocular Microscope. By Professor J. L. Riddell, of New Orleans.

It is proper to premise, that some brief notices of the Binocular Microscope have already appeared in *Silliman's Journal*, and elsewhere. I now desire to submit a few remarks and explanations to the members of this American Association, and at the same time to exhibit different forms of the instrument, so that the members interested in the microscope may form a definite opinion of the value and utility of the improvement.

The following diagram (Fig. 1) will serve to illustrate the method first devised and put in practice. It shows in longitudinal section the position of the objective-glass and prisms for producing binocularity.

Fig. 1.



O represents the object to be seen.

L, the objective combination; always brought as near as practicable to the prisms.

A, A', two isosceles rectangular prisms of fine glass, in contact by the edges, which are somewhat ground away. The light entering the prism A, through the objective, suffers internal reflection on the hypothenuse A, and emerges from the prism in the direction of B. Entering the prism B, it is restored to its original direction. So likewise that part of the luminous pencil entering the prism A' emerges nearly parallel from the prism B'. The prisms B and B' are adjustable to different distances apart, and have likewise an axial adjustment in the plane of the section represented; the first, that they may be made to correspond to the interval between the two eyes of the observer; the second, that the direction of the rays, travelling from each point of the object through these prisms, may be such as will seem to the observer natural and unconstrained, and give clear, coincident fields.

In the smaller instrument exhibited, this arrangement is observed. Used without eye-pieces it gives a stereoscopic and perfectly satisfactory result. This instrument was constructed for a dissecting microscope; I use it with lenses, whether plain, doublets, or achromatics, from $\frac{1}{2}$ to 3 inches focal length. The image is erect and orthoscopic. Objects can be viewed as opaque or transparent, and there is attached to it a flexible pipe, connected with a delicate cylinder and piston, which in one respect is made equivalent to a third hand. Tightening a screw, and taking the ivory termination of the flexible tube in the mouth, the focal distance of the instrument can be varied at pleasure with the breath. In very minute dissections, where two hands are simultaneously employed with hook and needle, I have found this method of holding a focus of the greatest utility and convenience.

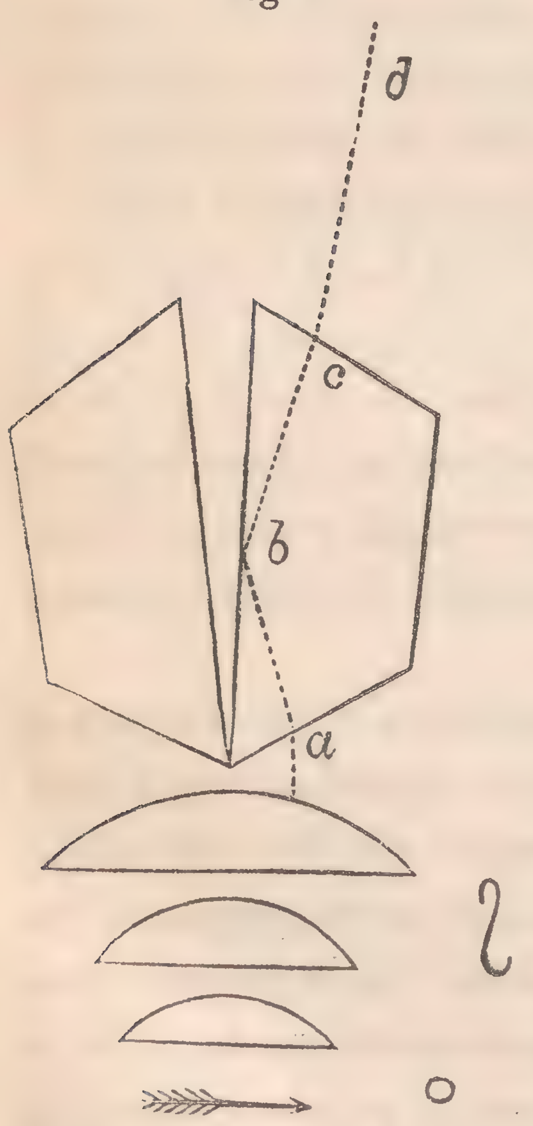
If over B and B' single oculars be placed, the binocular vision is found to be pseudoscopic; that is, depressions appear as elevations, and elevations as depressions. With erecting or double eye-pieces, analogous to the terrestrial telescope, the vision again becomes orthoscopic. On this account I prefer to reserve this form of instrument for use without eye-pieces, in the manner explained, and to construct the compound binocular microscope, on a new plan, which I will shortly explain.

Binocular Magnifier.

I have found that, for the magnifying-glasses used by artists and naturalists, glasses having a focal length of one or two inches or more, a less complex and more economical arrangement can be adopted; namely, the reflecting surfaces, *A A′* and *B B′*, Fig. 1, can be exchanged for pieces of common looking-glass, or plate-glass silvered. The reflections of the first surface are too faint to interfere materially with distinct definition. The two mirrors of the pair on each side of the nose are hinged together on the principle of the parallel rules. The whole arrangement is mounted something like a pair of spectacles, while the requisite lenses are adapted so as to be centrally attached when required. I regard the binocular magnifier as supplying a great desideratum to large classes of persons pursuing a great diversity of callings. The effects, so often prejudicial to vision, of inordinately using one eye, are thus avoided. A perfectly natural relief, or definition of bodies in depth, as well as in extension, is thus attained.

Binocular Compound Microscope.

Fig. 2.



In the larger instrument exhibited, only two prisms are used for subdividing the light, after its passage through the objective; and for directing the luminous pencils to the separate oculars. In this case, orthoscopic vision is produced by the ordinary single oculars. The light suffers *one*, instead of two reflections, as in the instrument before described. The arrangement of the prism is shown in section (Fig. 2).

O represents the object to be seen.

l, the objective, above and near to which are shown the two prisms.

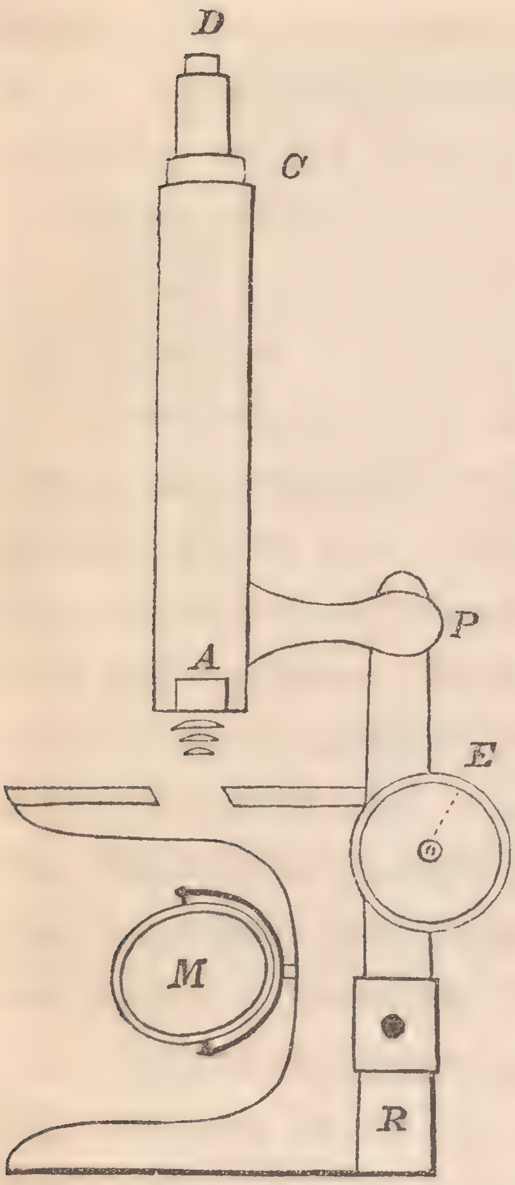
The internal reflection takes place upon the two long sides, which are in apposition at a small angle; which angle admits of adjustment in the plane of the section shown, the lower terminations always remaining in contact. The light through the objective, which impinges upon *a*, is that part of it which enters the

prism refracted to the left, so that it meets with the reflecting surface *b*. Suffering total reflection, it emerges from the surface *c*, where, from the necessary identity of the immergent and emergent angles, it is refracted to the left, so as exactly to compensate for its previous refraction to the left. This implies that the upper and lower angles of the prism are equal. In the instrument before you these equal angles are 45°. The ray of light in pursuing the path *a b c* suffers a minute chromatic dispersion; inasmuch as by the refraction and dispersion at *a*, the red, violet, etc. will be found somewhat separated at *c*. Thereafter, in travelling in the direction *c d* to the ocular, the red and violet will move in parallel paths, so that no further dispersion will occur. Upon a close scrutiny into this matter, I find that it does not practically lessen the sharpness of definition, unless an eye-piece of unusually high power be made use of. The minimum limit of angular definition, perceptible by the human eye, is about 45 seconds of a degree. The extreme dispersion occasioned by the prisms as above, may be kept handsomely within this limit. This can be shown both by calculation and by experimental demonstration. By making the equal angles of the prism near 85° or 86°, so that the immergence and emergence shall be at right angles to the glass planes, this theoretical dispersion can be avoided. But practically, in this case the usefulness of the prism would be destroyed by the interference of the light directly transmitted through without reflection. Prisms with equal angles of 60° will probably be found as appropriate as any.

It would be inappropriate to consume much of your time in explaining the mechanical details of this instrument. The following sketches will assist you to comprehend the essential peculiarities of a plain, firm, and comparatively simple stand, with all the most important adjustments.

Fig. 3 represents a side view of the instrument. The stage is immovable, being firmly supported, so as not to spring sensibly under considerable and sudden pressure; it extends six by four inches. The optical parts are supported by a stout triangular gun-metal bar, bearing rack-work and moving up and down by a pinion, terminating in large milled heads, one of which is shown at *E*. For the convenience of changing objectives, the arm carrying the optical apparatus has at *P* nearly a half-revolution, so as to carry it off the

Fig. 3.



stage. The prisms are at the bottom of a brass box at *A*. One of the oculars is seen, as fitting into an adjustable tube, *C*. A small, rectangular, equilateral prism is so mounted in a brass cap, as to slip at pleasure over the eye-glass. This little prism is adjustable in the plane of the drawing, on an axis transverse to this plane, so as to erect the image seen, and at the same time allow of its being viewed at any inclination between verticality and horizontality which may be convenient to the observer. It will be seen that the prism at *A* has the effect of erecting the image in one plane; while the small prism at *D* can be placed so as to erect it in the plane precisely transverse. Thus the movements upon the stage will be seen through the instrument as natural or erect; a condition essential to the convenient manipulation or dissection of a microscopic object. *M* represents the position of a concave mirror, or other apparatus for illuminating transparent objects. Two small mirrors will be found more satisfactory than one large one; as the operator can then easily secure a good light to each eye, which is sometimes difficult with a single mirror.

Fig. 4.

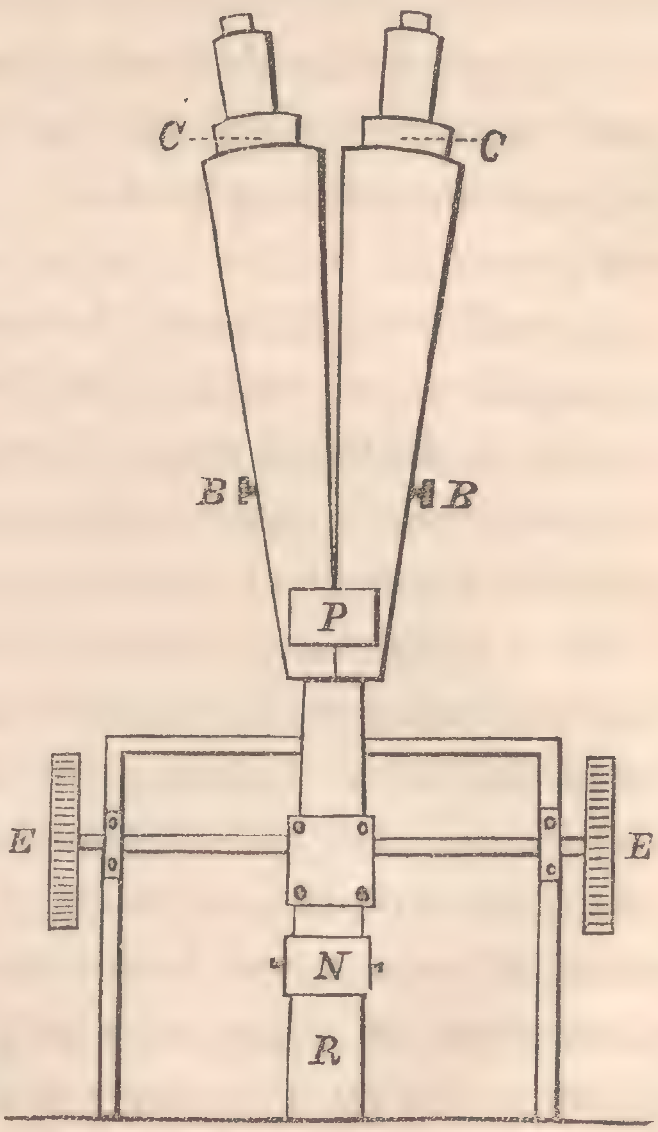


Fig. 4 exhibits a back view, the letters common to both referring to the same parts as in Fig. 3. Thus, *C*, *C* are the adjustable tubes into which the oculars fit. These tubes are hung upon axes, so that their inclination to each other may be varied; and the whole arrangement slides at pleasure, horizon-

tally, in order to adapt the distance to the eyes of different observers. *B*, *B* are the milled heads of the screws for the adjustment of the inclination of the prisms, as explained in connection with Fig. 2. *R* is a brass tube surrounding the box in which plays the triangular gun-metal supporting bar, before explained. Concentric with *R*, and movable thereon, is *N*, a short brass tube, carrying the illuminating apparatus.

Let the observer using this instrument carefully illuminate the object to be seen, then, after adapting the lines of vision to the natural requirements of the pair of eyes, and duly alligning and superposing the corresponding images, and carrying them into the corresponding parts of the two circles of light as defined by the diaphragms of the oculars, — and, lastly, regulating the local portion of the object from the objective, all of which can be readily accomplished by the various adjustments, — let him place two good eyes of equal power in the proper position near the eye-glasses, and a magnificent field will present itself to the sight. He seems to look through a circular window, or porthole, say two feet off and a foot in diameter, a foot or two beyond which his microscopic objects, perhaps seemingly hung in mid-air, stand out in all the boldness and perfection of relief, and with definiteness of position in width and depth, which he has been accustomed to realize without glasses, in the natural objects around him. It does not appear to him that any glass or other artificial medium is interposed between his eye and the objects seen. The vision fatigues him no more than does a landscape, or the inspection of the implements and objects on the table before him. A drop of water, teeming with algæ and living infusoria, looked into in this way for the first time, impresses upon the beholder, even though he be a veteran microscopist, a profound sense of the sublimity of Nature in her lesser spheres; and a vivid consciousness of beholding the microscopic world in a new and seemingly palpable condition.

By varying the inclination to each other of the luminous pencils entering each eye, the objects can be made to appear definitely nearer or farther off at pleasure. In these cases the parallax, or apparent angle subtended by an object, remains constant, while the apparent size varies of course with the apparent distance. Thus, a mite of a wheel-animalcule, one hundredth of an inch long, will perhaps appear to be a foot off and as large as a mouse; but bring the prisms nearer together, and tilt the oculars so as to correspond, and the image waxes

marvellously immense; and, taking a position, perhaps, apparently more than a hundred feet distant, this being, too small to be seen with the naked eyes, vies with the great whale of the ocean in size, wearing an aspect more awful to behold than the savage beasts of an African forest, and clearly exhibiting a complex transparent structure more unique and wonderful than the mind of man can well conceive.

This instrument, with its firm stand, broad stage, and erect images, is pre-eminently adapted for use, in prosecuting minute dissections, or the unravelling of minute structure of any kind. Opaque objects may be illuminated by the bull's-eye condenser; and transparent objects by two concave mirrors, aided by two diaphragms or screws, or one large concave mirror and two screws. At night, two candles may be used conveniently with one mirror. To illuminate for the higher powers, a single achromatic condenser suffices.

Almost any model or form of *monocular* compound microscope extant can be modified so as to become binocular, on the principle here explained in connection with Fig. 2. In one respect it would be convenient to adopt the trunnion-mounting of Spencer; the whole instrument might be tilted, so as to use conveniently the camera-lucida in drawing. This would detract from the firmness and simplicity of structure, so essential in a dissecting microscope, and add materially to the cost, a circumstance of importance to some who might wish to possess it.

As it is, the instrument can readily be braced up at an angle of 45°, at which angle Nachet's camera-lucida works in drawing with perfect satisfaction. If the same object be drawn, as seen through each ocular respectively, a difference between the two drawings is perceptible, similar to that between matched stereoscopic pictures; so that if then the two drawings be viewed, each with the appropriate eye, the natural relief of the object is reproduced. I have already suggested, in *Silliman's Journal*, the propriety of publishing such drawings, appropriately placed upon the paper, in illustration of natural history and histology.

3. On a Singular Case of Internal Fringes, produced by Interference in the Eye itself. By Professor Joseph Lovering, of Cambridge.

If a beam of sunlight be reflected into a room through a long and narrow slit in a window-shutter, and an opaque body of similar shape be placed in its range, so as to cast its shadow upon a wall opposite to the window, this shadow, it is well known, instead of being restricted to its geometrical limit, is ornamented at its borders with colored fringes, which, upon micrometric examination, show that a portion of light has spread within the geometrical line of shadow, and also that a portion of light outside of this line has been obliterated. The phenomenon observed without this line is known under the name of *external fringes*, and the phenomenon observed within this line is described under the phrase of *internal fringes*. If the body which throws its shadow upon the wall is long and slender, as a fine needle or thread, the internal fringes intrude into the very centre of the shadow, so that this part, which commonly is the darkest, is now often highly illuminated. It is hardly necessary to add, that these infringements upon the primary law of shadows are so fully explained by the undulatory theory of light, analytically applied to the case, that some of the most delicate features of it were predicted with mathematical precision before they had been included in recorded observations. It will be observed that a very small beam of light, with a linear or circular section according to the shape of the body to be placed in it, is an essential condition for the production of fringes. Otherwise, the effect produced by one part of the beam will be obliterated by the superposition of a complementary action from another part of the beam.

Now I have found that these internal fringes, which are commonly exhibited by means of Fresnel's delicate and costly contrivances, can be produced, in some instances at least, with no other apparatus than what is furnished by the eye itself. It is only necessary to direct this organ towards the fine threads of light which penetrate the thick foliage of summer. When this is done, I have seen the internal fringes projected on the retina as distinctly and as neatly as when the eye looks through Fresnel's *micrometric eye-piece for diffraction*. In

pursuing the subject, I have come to the conclusion that the fringes originate from fine opaque motes, sometimes straight and sometimes of complex curvature, which are probably floating in the aqueous humor of the eye. These fine lines, taken in connection with the slender beam of light produced by the foliage, furnish all the required conditions for success in these experiments. That the lines which cast their fringed shadows upon the retina are floating in the humors of the eye, I infer from the fact that the systems of fringes are in motion, — coming into the field on one side and sweeping out on the other; and that, by a proper pressure, the direction in which they enter and leave the eye can be controlled at will. I will conclude this communication with four observations: — 1. I suppose that no microscopist will be surprised at a result which indicates the existence of foreign matter, animated or otherwise, in the proper fluids of the eye. 2. It is curious to observe that the interposition of this fine matter in front of the retina, which others have recognized by its *unfringed* shadow, so far from interfering with ordinary vision, cannot even be recognized except in extraordinary circumstances. 3. It is highly important, in the present community and at the present day, to sift out from the incongruous heap of subjective or spectral phenomena those which admit of a decided physical explanation, leaving behind those only which must be referred to the imagination. 4. It seems worthy of consideration how far, if at all, the action of interference, either within or without the eye, may vitiate the apparent structure of a body, as seen under the microscope.

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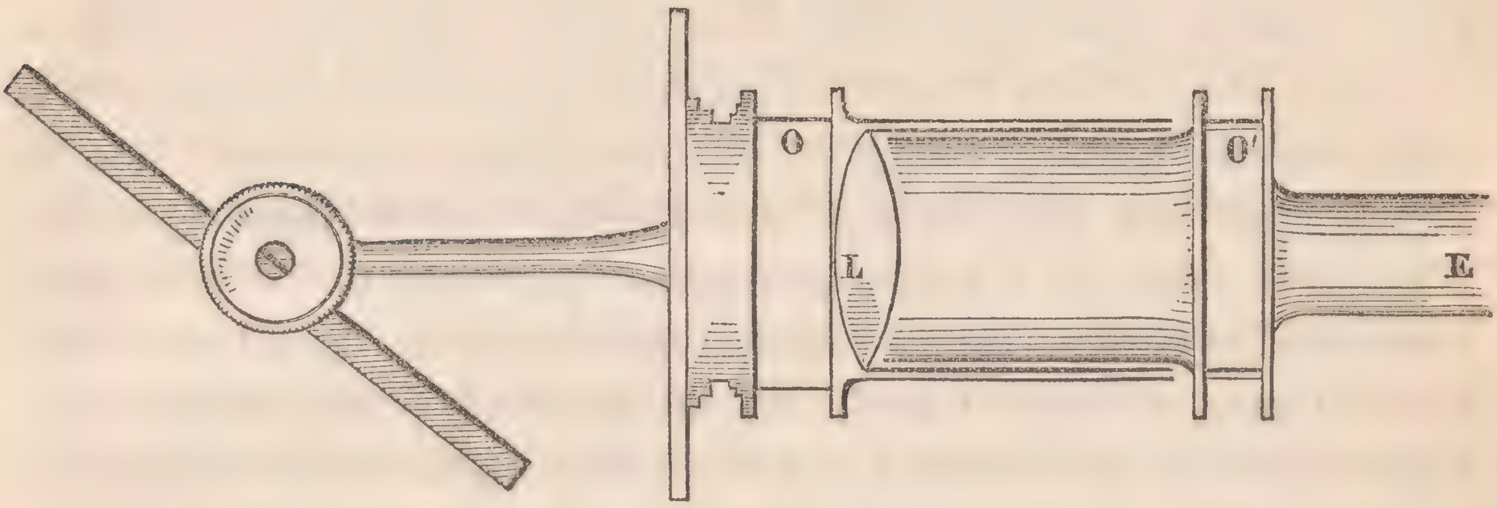
4. On a Modification of Soleil's Polarizing Apparatus for Projection and other Experimental Contrivances. By Professor Joseph Lovering, of Cambridge.

1. I shall first give an account of some changes, and, as I believe, improvements, in the construction of Soleil's apparatus for projecting on a screen the chromatic phenomena of polarization, and performing experiments the most novel as well as the most beautiful in the whole vast range of Optics.

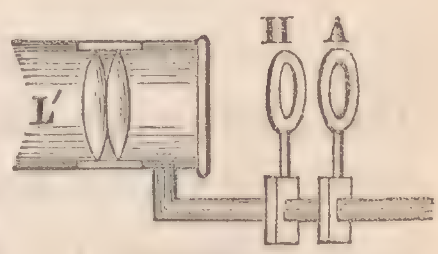
The apparatus of which I speak, as made by Soleil, is described in Pouillet's *Traité de Physique*, and elsewhere; and a sectional view of it is given on Plate XXXIV., Fig. 21, of the book first mentioned. I

Fig. 1.



here present the apparatus as I have modified it. Soleil's plan is well adapted to many experiments. If the crystal is small, it is placed near the focus of the large lens, which condenses the light upon it and illuminates it strongly, so that the colored rings, black crosses, &c. developed by the analyzer may be projected upon a distant screen on an enlarged scale. But whenever these experiments are to be repeated with crystals of low crystalline power, as quartz, for example, some modification of the apparatus is necessary. Otherwise, it will be impossible to see anything more than the colored centre without the rings. A more diverging beam of rays is required for the rings. Thus have I been led to the construction of another eye-piece, to be substituted in such cases for that which accompanied the original apparatus. It is represented in Fig. 2. The two small lenses, L′, having with their united forces a focal length of less than an inch, receive the converging rays as they come from the large lens, L, and make them still more convergent. It is necessary that the quartz should be adjusted very near the focus of the rays, and also that the analyzer should be placed as closely as possible in front of the quartz. Otherwise, this highly divergent pencil of light, after it leaves the focus, will not be able to get through the small area of the crystal and analyzer, and therefore the whole field of view will be curtailed. The crystal and analyzer are held by separate rings, which are supported on uprights

Fig. 2.



which slide independently upon a horizontal bar, and the two rings may, therefore, be pushed as closely together as the thickness of the crystals will allow.

In Soleil's apparatus, there is an opening, such as is seen at O of Fig. 1, similar to that made in magic-lanterns, and for the same purpose, namely, the introduction of the frame containing the object to be magnified. When Soleil's apparatus is used for the inspection of large objects, such as pieces of tempered glass or artificial selenite figures, they are introduced at this opening, and in these cases the large lens serves as a magnifying-glass, and there is no condensing lens to help the illumination. This defect would not be felt with very large objects, as large, that is, as the lens itself; because the object itself would receive immediately as much light as the lens could throw upon it. But the objects to be examined are seldom, if ever, of this magnitude. Hence, much of the light which might otherwise be saved, is lost for want of a condensing lens. In order that the apparatus may retain all the advantages which belong to it in its original form, I have retained the opening O, which may still be used in any cases where it is preferred. But the tube which carries the large lens has been divided at the middle of its length, and one part made to slide in the other. Another opening, O′, similar to that in the original apparatus, has been joined to one end of the interior tube. By pushing more or less of this tube into the next exterior one, the objects placed in the new opening can be put at smaller or larger distances from the principal lens, which is to this opening an illuminating lens. The best distance, in any given case, is that which makes the section of the cone of rays by the object equal to that part of its own area to be examined. When large pieces of quartz, the tempered glass, or selenite figures, are examined under these circumstances, the superiority of the illuminating arrangement is very decided, and allows of the picture being displayed on a much larger scale than would be otherwise practicable.

2. Since Newton's prime experiment on the prismatic dispersion of white light, many contrivances have been used to produce the synthetical counterpart to this grand result of experimental analysis. The various methods of uniting the prismatic colors again, so as to restore the white light, are enumerated by Moigno, in his *Repertoire d'Optique*, Vol. IV. p. 1370, as they are given by Dove.

It is desirable to obtain the original pure prismatic tints, and then unite the rays, if possible, by some change independent of refraction, and involving no obscure process. Von Münchow does this by giving a reciprocating motion to the prism, and Steinheil by turning a prism, one side of which is ground or blackened so as to intercept the light, rapidly round an axis parallel to the refracting edge. In either case, as soon as the motion acquires a certain velocity, the colored spectrum vanishes, and a streak of white light appears in its place. This subjective mixture of the colors may be effected in a cheaper manner, by any one in possession of a water-prism, as follows. The light is introduced through one of the inclined glass sides of the prism, at such an angle as to emerge from the upper surface of the water. If the prism stands firmly upon a table, a spectrum will be projected upon the ceiling of the room. But a moderate tap with the finger upon the table will communicate a rapid vibration to the hollow prism, and thence to the water contained in it, and the little change in the refracting angle which ensues will make the spectrum dance backward and forward in the direction of its length. As soon as the dance begins, the spectrum, which hitherto had been of the usual colors, is converted into a long streak of white light.

3. The only remaining experiment to which I shall allude in this communication carries us out of the province of Optics, and into that of Acoustics. It is well known that, when a tube filled with air is skilfully blown by the mouth at the embouchure, or a vibration in the enclosed column of air is otherwise excited, it is capable of rendering, without any fingering and without holes to finger, a certain series of sounds, which are called *harmonics*. In the humming-top, we may presume that the reaction is the same between the air inside and outside of the top, as it is in the musical pipe : the only difference being, that in one case the air is driven by the mouth against the lip of the embouchure, and in the other the lip of the top is made to strike against the air. I have not been able to find any allusion to the question whether the note which the top yields varies with the force of the blast which the top itself gives by its own motion. But I have succeeded myself in producing two of the harmonics, that is, one besides the fundamental sound. Success in this experiment requires that an extraordinary velocity should be given to the top, greater than is possible except when the top is small. But if a small top is

started with great energy, it gives out at first a high sound. As the velocity slackens, this sound dies out, and the top is silent for some time. Afterwards, as the motion diminishes, another and lower sound starts up, which is the one commonly heard in humming-tops, and the only one possible to excite by the usual way of starting the top, if it is large. This curious experiment may be worth recording, as carrying one step further the analogy between the theories of the sounds of the humming-top and of other wind instruments.

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5. Strictures on the Mechanical Explanation of the Zigzag Path of the Electric Spark. By Professor O. N. Stoddard, of Miami University.

The zigzag path which the electric spark usually describes is one of the most marked among the varied phenomena of this wonderful agent. Says Biot, "The zigzag form of the electric spark is well explained by the compression of the air in front, which turns it from its course; the air is again compressed, and the spark is deflected in a different direction." This is a type of most of the explanations given; and which, for distinction's sake, I call mechanical. It assumes that the spark in its passage through the air, bears to it the same relation that any other body would under the same circumstances.

The same method of solution used in estimating the disturbing effect of an elastic medium upon a body moving through it, must be used here.

The laws which govern the deflection of a body, by a lateral force, from the rectilinear course in which it is moving, also govern here. The terms of the explanation shut us up to these conditions, and we are not allowed in the investigation to introduce suppositions, or deduce conclusions which virtually cancel the premises.

If the question is placed entirely, as it is in the above explanation, within the province of mechanics, then by the laws of mechanics must it be judged, and by them it must stand or fall.

The explanation, that the zigzag path of the electric spark is owing to the compression of the atmosphere in front, involves two suppo-

sitions: — 1. That the electric agent, or that which produces these phenomena, is itself a body, solid, liquid, or fluid, and independent of the medium which it traverses. 2. That the spark is not symmetrical.

That electricity is a material body, as distinct from and independent of other forms of matter as they are of each other, it is not necessary now to affirm or deny.

That there is in every electric discharge a mass of matter passing from one point to another, may be questioned without the charge of extravagance. Suffice it to say, that the supposition which these strictures contemplate leaves us no option. It must be matter of some kind, capable of accumulation within definite limits, — of being discharged with amazing velocity and intense energy. It must be inert, offering resistance to a change of state; in a word, it must have all the essential properties of matter. But granting that it is a body, and that the spark is caused by its motion through a resisting medium, then it follows that the spark is not symmetrical in form.

A body moving in a homogeneous medium, of uniform density, would not be deflected from a right line unless the resistance were greater on one side than on another.

A perfect sphere in motion in an atmosphere of uniform density could not be changed from its original direction by the resistance of that atmosphere, however great it might be. The resistance on the right of the line of direction would be counterbalanced by an equal amount on the left; the resistance above by an equal amount below.

As deflection from a right line cannot take place in a moving body, unless there be a greater force on one side than on another, and as no such excess of force exists in the present case, therefore deflection could not occur.

The same would hold true of any regular body, provided the line along which it moves coincides with any principal axis around which the parts are symmetrically arranged.

If, however, the medium be composed of strata of varying density, as is the case with our atmosphere, then the electric spark, provided its course be oblique across the strata, would not describe a straight line.

It seems hardly necessary to state here, what is so obvious to every one possessing any knowledge of the general principles of mechanics,

that the straight line becomes, under these conditions, a curve. It could not possibly be zigzag.

But suppose we grant that the spark is not symmetrical, that it may change its form at any point and at any number of points along its path; even then the line could not become a broken one. The excess of pressure on one side of the spark, arising from the condensation of the atmosphere, would indeed produce a deviation, but the case then becomes simply that in which a body is acted upon by an impulsive force in one direction, and a constant force in another; a curved path will inevitably be the result. A zigzag path would in this case be as much a transgression of physical laws, as the supposition of some of the ancient philosophers, that a cannon-ball, in its descent to the earth, fell by a succession of steps.

The only remaining supposition, that the spark is turned from its course by encountering strata of varying density, neglects the fact that these variations constitute a regularly increasing or decreasing series; and as the number of strata becomes indefinitely large, the deflections will therefore be indefinite in number, and, each being indefinitely small, a curve will be formed.

I have not deemed it necessary to enter at all into details in presenting this question, and for the obvious reason, that the principles involved are so simple, and their application so direct, that a mere statement of them is all that is needed to bring the subject fully within the comprehension of every one.

I have passed over one case which perhaps does really exist, namely, that the spark is not only impelled from a point in a given direction, but that the point towards which it is impelled also attracts. This was neglected because the two forces act in the same line, the one impulsive, the other attractive, and may therefore in practice be treated as one.

If these forces are found to be variable, then the curve would be changed in form, but it would still be a curve.

If, then, we grant all the premises embraced in the explanation stated at the beginning, — namely, that the electric spark is a material, independent body, and that it is not symmetrical in form, — even then the path cannot be broken or angular; if any deviation from a straight line occurs, it must be in curves.

The only importance which I attach to a decision of this question

is, that the relation which it assumes to exist between the spark and the medium in which it is produced is not the true one.

It is not my intention to attempt, at this time, any explanation of this peculiarity in the electric discharge. The period has not come for anything more than probable conjecture. The indications are, indeed, clear enough as to the direction in which the truth is to be sought. That it will be found ultimately, that the relations of electricity to matter are of a molecular character, is more than probable ; but the precise condition of these relations is yet to be learned. Professor Faraday's statement, though not an explanation, may embrace the elements of one, to be modified and wrought into form by succeeding laborers. The statement is briefly this : " That the path of the spark is the resultant of all the inductive forces acting through and by the instrumentality of the medium in which the discharge takes place ; and that whatever cause or causes produce irregularity in the action of these forces, would produce corresponding irregularities in the resultant."

An observing mind cannot fail to perceive that all the lines of discovery in electricity, and in those sciences which hold an intimate relationship to it, appear to converge to some remote point. Whether that point is ever to be reached, remains to be seen.

The way seems to be preparing for some sublime generalization, which shall include these sciences under some great law, as general as that which holds the heavens and earth in its grasp.

Will it be out of place to venture here the prediction, that the individual who shall eliminate, from the vast collection of materials, this law, and by it explain, not only what is regular, but what is seemingly irregular, will stand side by side with the great Astronomer ? and the hour of that discovery, it is not fanciful to suppose, will be signalized above every other period in the annals of science. Is this exaggeration ? Wait a little : time will show.

III. PHYSICS OF THE GLOBE.

1. ON THE TIDES AT KEY WEST, FLORIDA, FROM OBSERVATIONS MADE IN CONNECTION WITH THE UNITED STATES COAST SURVEY. By A. D. BACHE, Superintendent. [Communicated by authority of the Treasury Department.]

HOURLY observations of the tides were made at Fort Taylor, Key West, from the 1st of June, 1851, to the 31st of May, 1852, by Mr. J. W. Goss, and other *employés* of the Coast Survey assisting him The tides ebb and flow twice in the twenty-four hours, but the diurnal inequality in height is relatively large, amounting at a mean to 0.55 foot, and reaching in extreme cases 0.83 foot. The mean rise and fall of the tide being about 1.4 feet, a knowledge of the laws of the diurnal inequality by which successive high or low waters may differ is very important. The corrected establishment of Key West is $9^{h.}$ $22^{m.}$. The curves of Plate I., Nos. 1, 2, 3, 4, and 5, show the normal character of the tides at the maximum and zero of the moon's declination, at the syzygies and quadratures, and at a mean of declination and six hours of the moon's age. There being two tides in the lunar day, the observations admit of discussion by the ordinary methods, while the large diurnal inequality in the height of high water renders it desirable to pursue the mode which I have applied to the tides at Cat Island, Louisiana, and Fort Morgan, Alabama. The reductions by the ordinary methods thus become the tests of those by the other mode. The former were made under my immediate direction, by Lieutenant Richard Wainwright, U. S. N., and Mr. M. H. Ober, U. S. Coast Survey, and the latter by Mr. W. W. Gordon, assisted by Messrs. Mitchell, Homans, and others of the Coast Survey.

The *half-monthly inequality* in time and height as deduced by the usual method is shown in the following table, No. I., in which the first column contains the moon's age, the second the mean luni-tidal interval corresponding, and the fifth the height.

The mean interval for this table is $9^{h.}$ $22^{m.}$, corresponding to the epoch of the moon's age of $24^{m.}$, showing that the transit *E* (of Mr. Lubbock's notation), and not *F*, should be used in the reduction for theoretical purposes.

No. 2 Minimum S. (Quadratures)

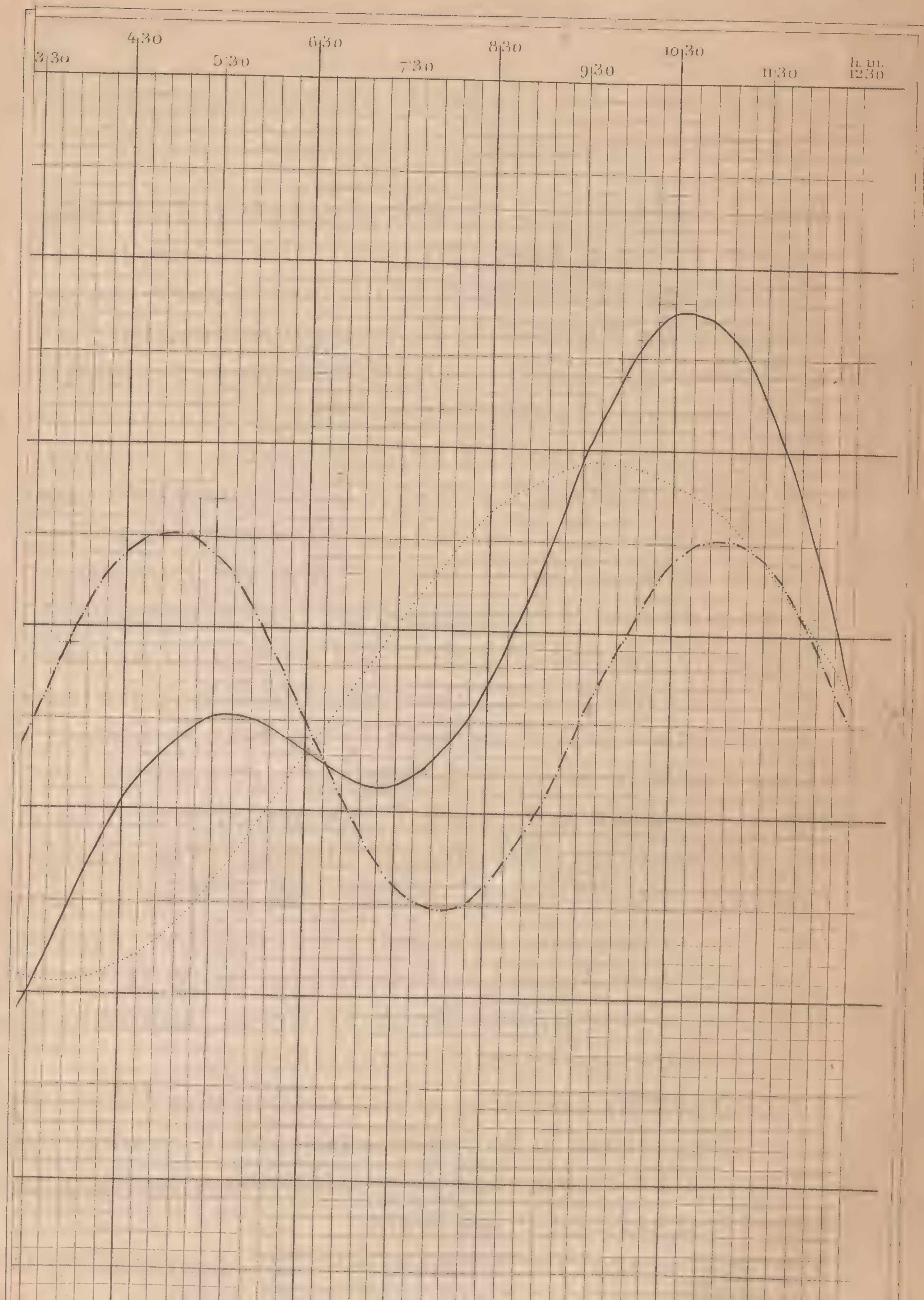


Plate No. 1

Type Curves, Key West, Max. Decl. of Moon

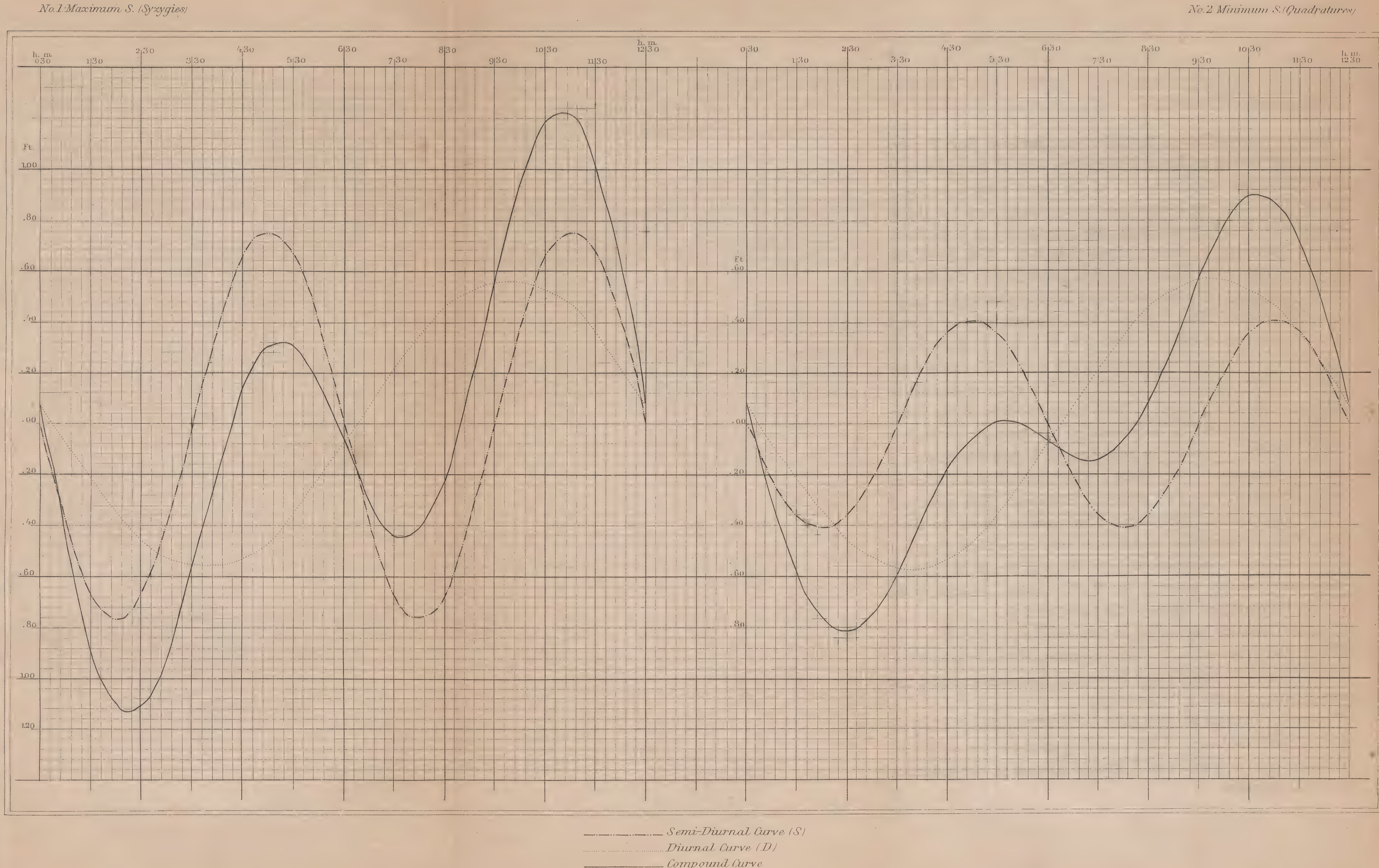


TABLE I.

Half-monthly Inequality of Tides at Key West from One Year's Observations.

1	Interval. 2	3	4	Height. 5	6	7
☽'s age.	O.	T.	O.—T.	O.	T.	O.—T.
h. m.	h. m.	h. m.	m.	ft.	ft.	ft.
0 30	9 21	9 21	0	6.34	6.34	.00
1 30	9 5	9 7	2	6.31	6.32	.01
2 30	8 51	8 54	3	6.25	6.26	.01
3 30	8 47	8 46	1	6.17	6.16	.01
4 30	8 50	8 45	5	6.08	6.06	.02
5 30	8 54	8 58	4	6.00	5.97	.03
6 30	9 22	9 25	3	5.94	5.94	.00
7 30	9 52	9 49	3	6.00	5.98	.02
8 30	9 59	10 0	1	6.02	6.08	.06
9 30	9 59	9 58	1	6.12	6.18	.06
10 30	9 53	9 48	5	6.22	6.27	.05
11 30	9 35	9 35	0	6.30	6.33	.03
	9 22					

The comparison between the results of observation and those from the formula for the half-monthly inequality is shown in the fourth and seventh columns, the fourth referring to the interval, and the seventh to the height. The difference in the mean is inappreciable, and at a maximum is but five minutes of interval and six hundredths of a foot of height.

A graphic comparison is made on Plate II. The value of the constant (A) of the formula for the interval $\text{Tan } 2\psi = \frac{(A) \sin 2\phi}{1 + (A) \cos 2\phi}$ is 0.325, and of E in the formula for the height

$$h = D + E \,.\, (A) \cos (2\psi - 2\phi) + E \,.\, \cos 2\psi$$

is 0.620.

The values of the *diurnal inequality* of high and low water, both in time and height, were obtained by comparing the mean value of the interval and height for the first and second six months with the individual values; they followed closely the law of change with the moon's declination. The inequality in height of high water at a mean is to that of low water as 79 to 61.

As the observations were only made hourly, and the inequality in the interval of high water is small, the minute changes from day to day could not be expected to show themselves. The inequalities were grouped, according to the different declinations of the moon, into fourteen periods, and the approximate formula given by Mr. Lubbock for

the variation from the mean was applied. The agreement with theory, as shown in the annexed table, is very close. G was taken as 1.15.

TABLE II.

Comparison of the Diurnal Inequality of High Water at Key West, with the Formula

$$d\,\psi = \frac{G \tan \delta'}{1 + (A) \cos 2\,\varphi}.$$

Moon's Declination.	Diurnal Inequality.		Difference.
	Observed.	Computed.	
° ′	′	′	′
3 45	13	15	— 2
7 15	25	29	— 4
11 30	48	47	+ 1
15 45	61	64	— 3
18 45	74	78	— 4
20 45	88	87	+ 1
21 30	100	91	+ 9
21 45	95	92	+ 3
20 15	84	85	— 1
17 30	83	72	+11
13 45	52	56	— 4
9 15	37	38	— 1
5 15	25	21	+ 4
2 45	7	11	— 4
			—23
			+29
			52

The inequalities of time of high water were also arranged according to the moon's age, but the agreement of observation with the formula is not as close as in the former instance, as must be the case from the small number of observations, and the variation of the inequality, following chiefly the law of the declination. The law of change is still evident in the grouping, and the plus and minus quantities balance nearly.

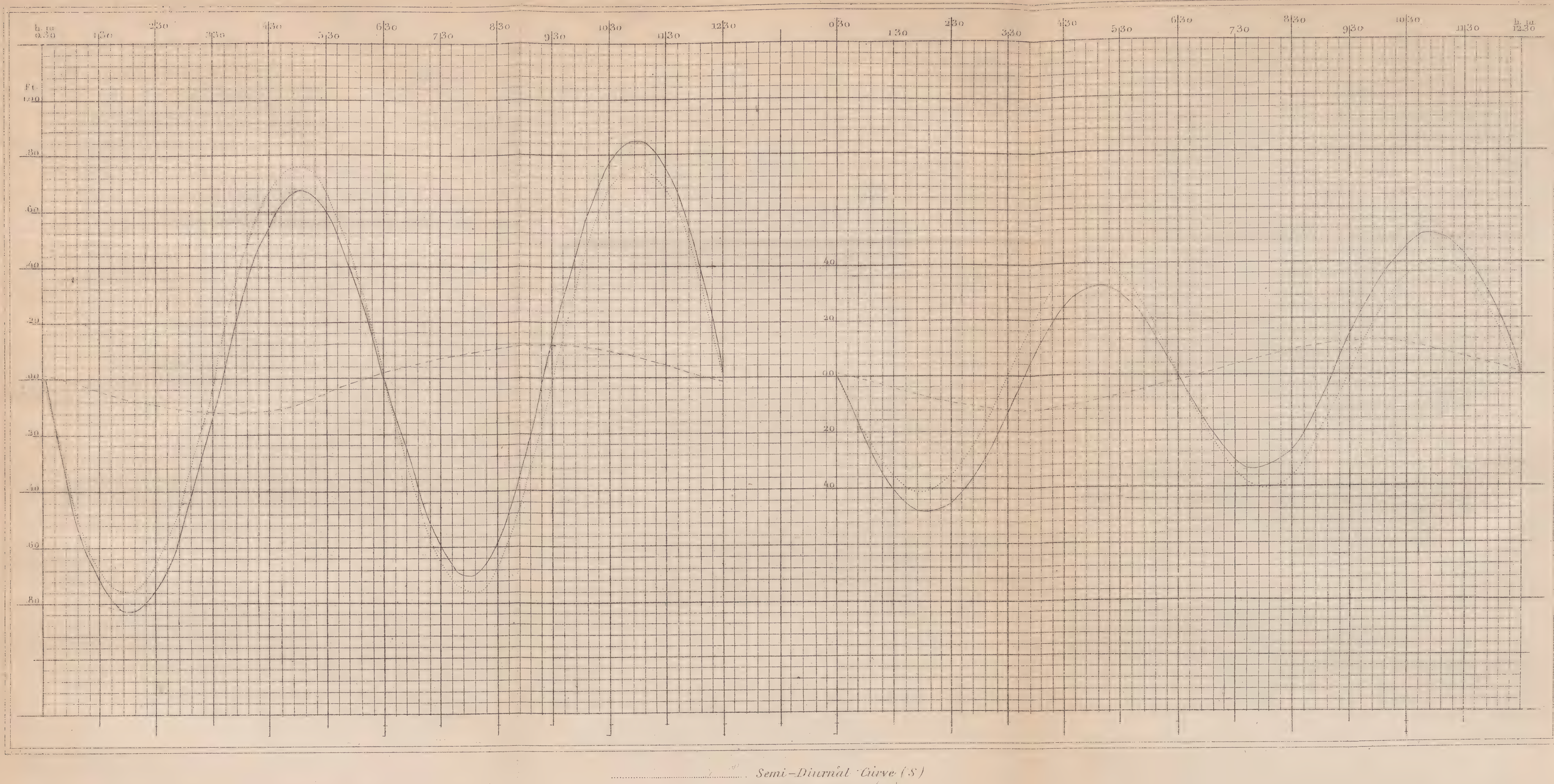
The discussion of the diurnal inequality in height will be resumed in referring to the diurnal wave, after noticing the decomposition of the curve of observation into a semi-diurnal and diurnal curve.

Decomposition of the Curves of Observation.

As in the discussion presented for the Cat Island tides, the curves of observation at Key West were decomposed into two, one representing the semi-diurnal, and the other the diurnal tide. The interval (E), which was in the former case assumed to be constant, was here treated as variable. The observed ordinates, being referred to the

Plate No.1 (bis)

No. 3. Maximum S. (Syzygies) *Type Curves Key West. Zero Decl. of Moon* *No. 4. Minimum S. (Quadratures)*

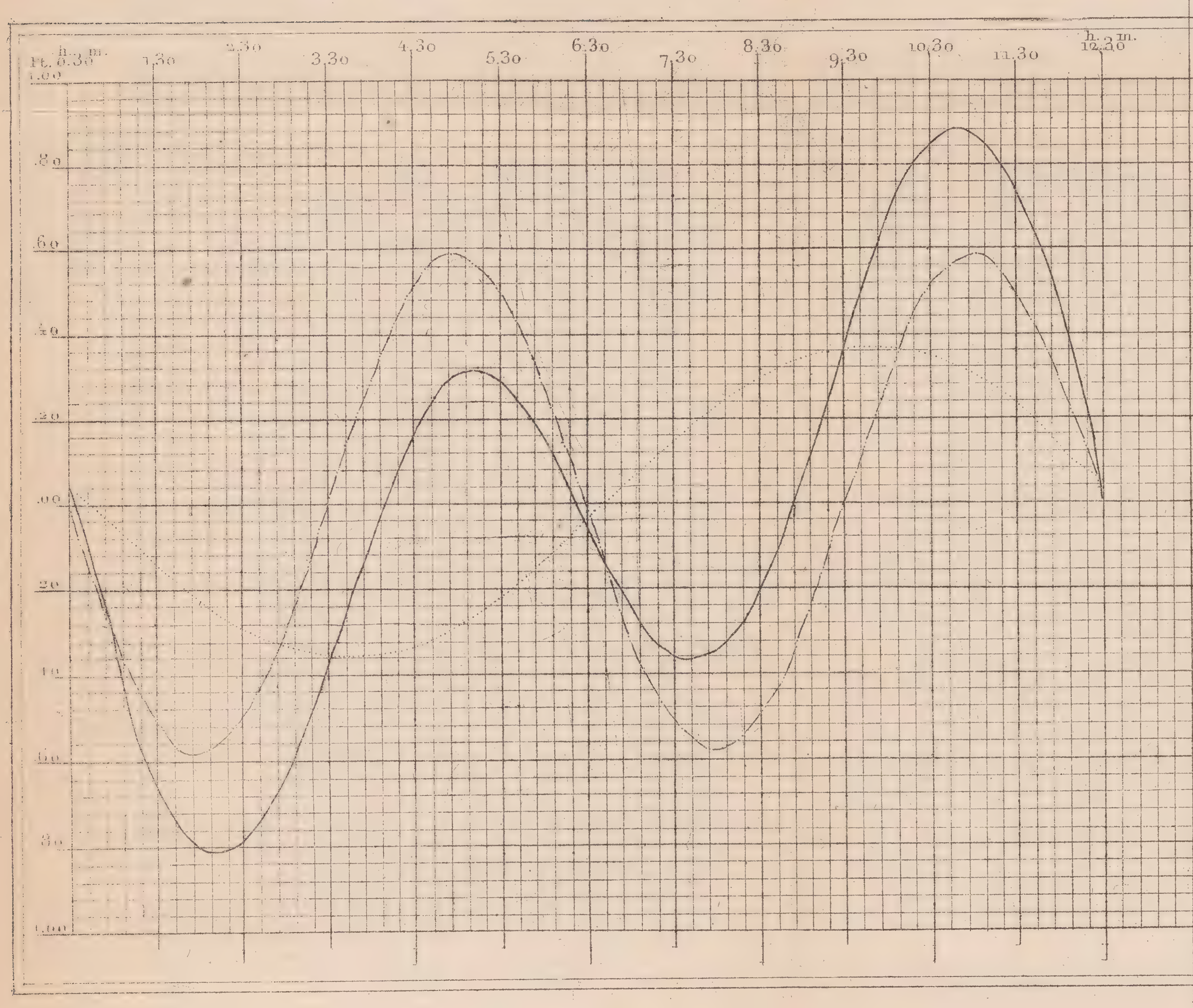


Semi-Diurnal Curve (S)
Diurnal Curve (D)
Compound Curve

Plate 1 (tris)

Type Curves, Key West. Mean Decl. of Moon

No.5 *Mean S.* *Mean D.*

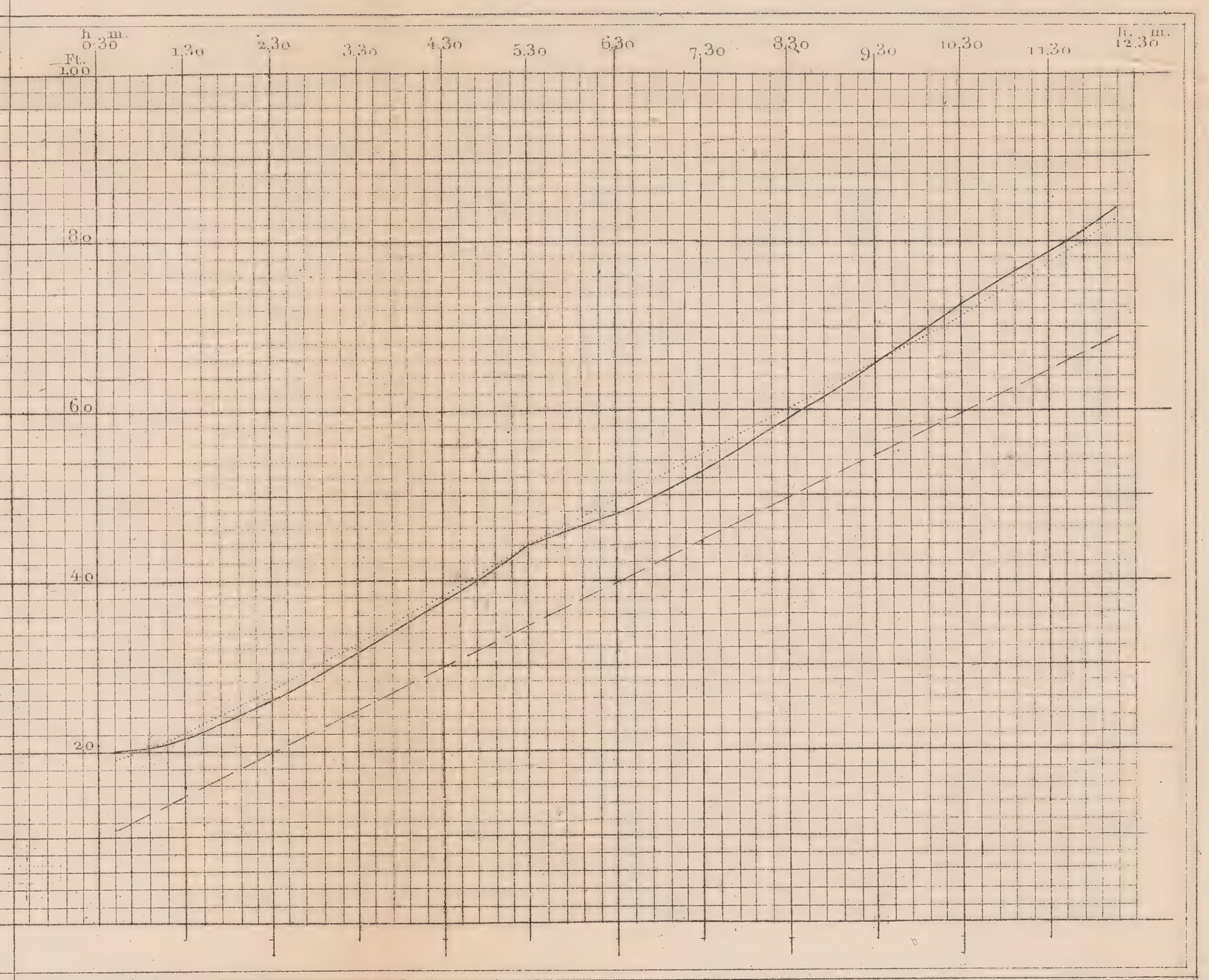


Semi-Diurnal Curve (S)
Diurnal Curve (D)
Compound Curve

Plate No.3

Whole year 1851-2

Comparison of Diurnal Inequality from observation and discussion of Curves with Moon's Decl.



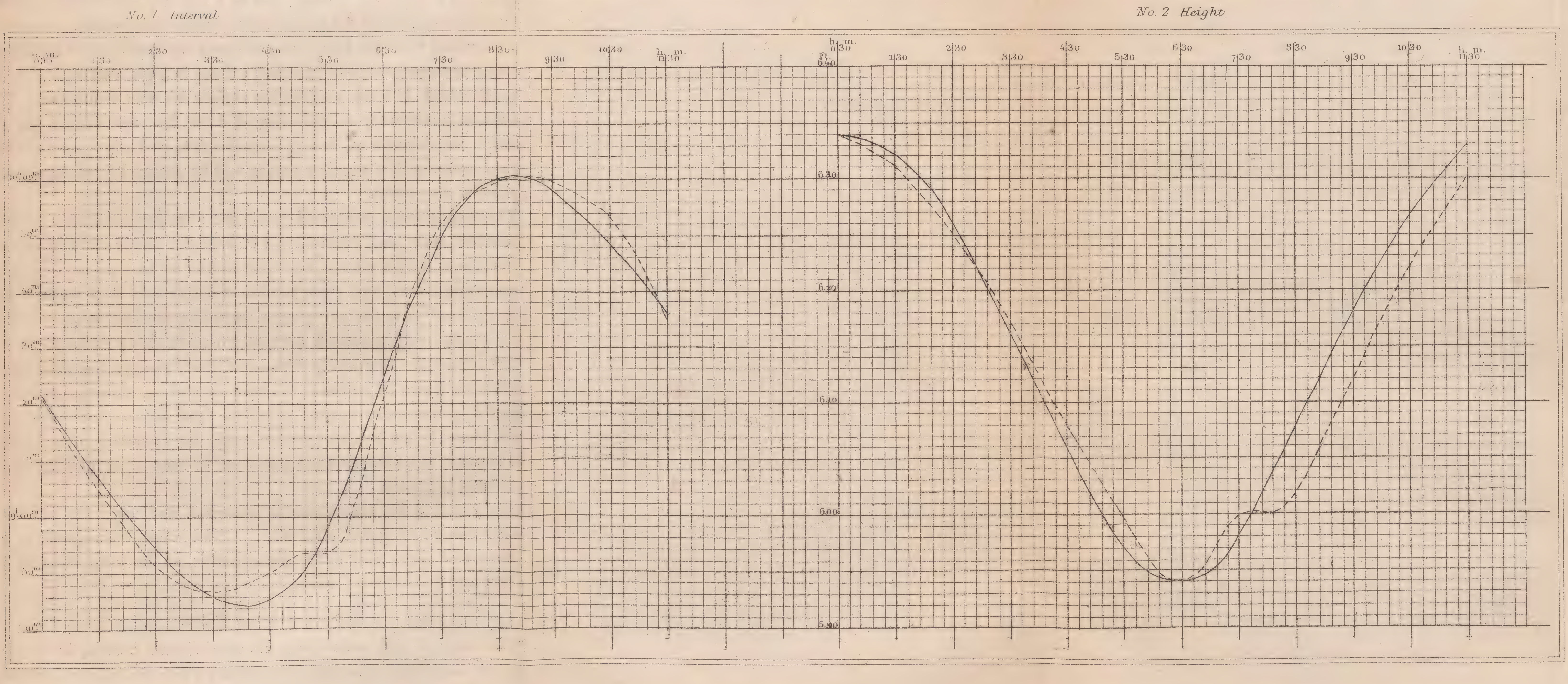
Observation
Discussion
Sine of Twice Moon's Decl.

Plate No. 2

Comparison of half monthly inequality in interval and height from theory and observation

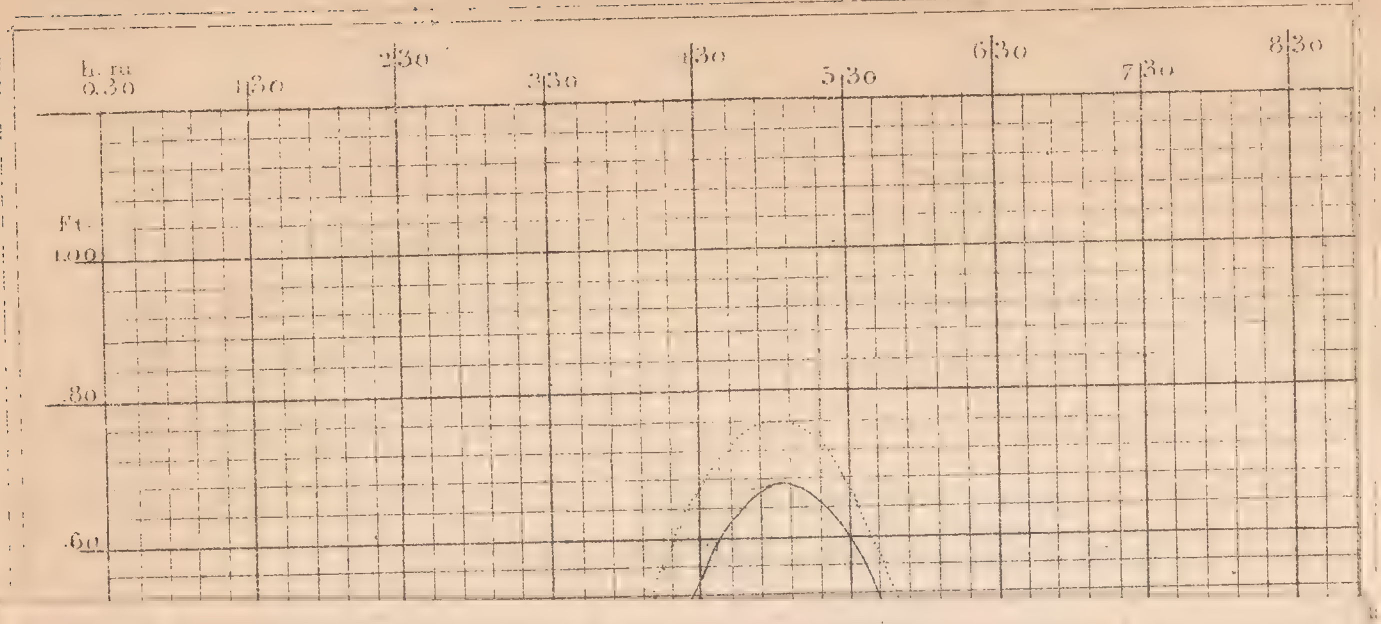
Whole year

No. 1 Interval *No. 2 Height*

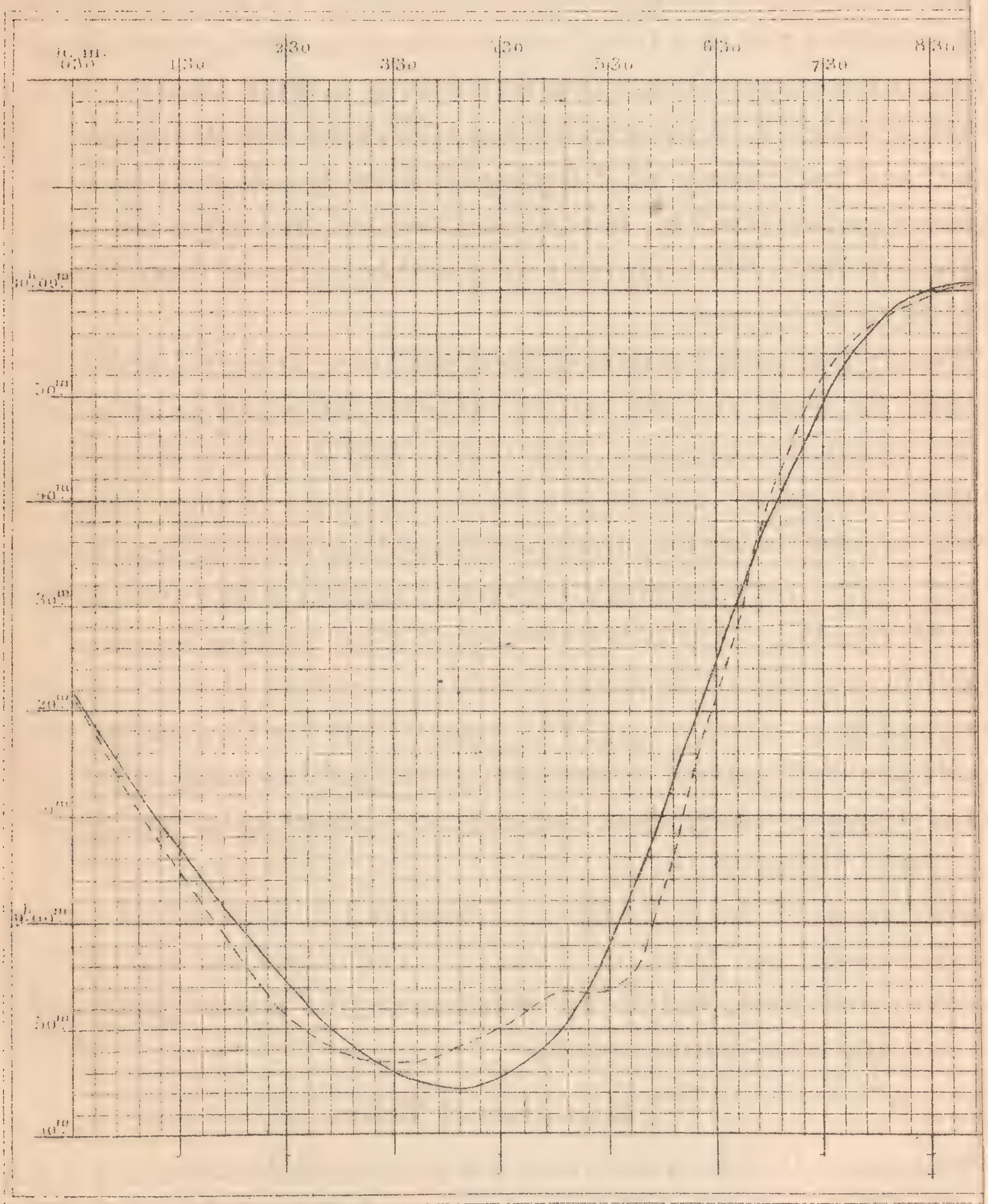


Theory
Observation
Theory
Observation

No. 3. Maximum S. (Syzygies)



No. 1 Interval



Theory

Observation

mean of high and low water of the day as a zero, were tabulated, and the maximum ordinates, S and D, of the semi-diurnal and diurnal curves of sines found, taking (E) generally at its mean value. From these the ordinates of each curve for the several hours were obtained, and thence the ordinates of the compound curves. These were compared with observation, and E was next made to vary until the value was found which gave the sum of the differences of computation and observation, without regard to sign, the smallest.

It was next intended, treating this as a first approximation, to take a different zero-point for the semi-diurnal curve; but the labor necessary has prevented this idea from being followed up thus far, and the agreement of the computed and observed curves is quite satisfactory in the cases in which E is not varying too rapidly for safe deductions. The labor and uncertainty of deducing E from the observations, in the manner just referred to, is very considerable, and after one full comparison made in this way, the values will be deduced from theory, and applied to the curves.

The approximate compound curve was next projected on a diagram of suitable scale, and the outline cut from the paper so as to apply it to the curve of observation, and thus to find its best position in reference to that curve, and to determine the times of high water. The work referred to in the paragraphs preceding the last is mechanical; but this latter requires much judgment, and has been executed by Mr. W. W. Gordon. Supposing that some discrepancies observed might result from a sort of personal equation in making these comparisons, a second person was engaged to repeat them for verification, and the result showed that the comparison could be depended upon in individual cases to within about five minutes in time in the position of the maximum ordinates.

Specimens of the accordance of observed and computed curves in normal cases are given on Plate I. The cases correspond to the greatest and least, and to the mean values, of the magnitude of the diurnal and semi-diurnal waves.

Semi-diurnal Tides.

The times of high water from the semi-diurnal curves, being taken from the diagrams, are subject to an error, which Mr. Gordon estimates at from four to five minutes; this, however, does not appear in the final results, which agree as well with theory as those for the heights not subject to any such error of estimate.

The luni-tidal intervals and heights found were tabulated according to the moon's age, as in the following tables, which contain the result for the first and second six months of the year, and for the whole year. The fourth and seventh columns contain respectively the differences in the interval and height from the curves, and from the formula for the half-monthly inequality referred to in the previous part of this paper.

TABLE III.

First Six Months.

Moon's Age.	Interval.			Height.		
	O.	C.	Diff. O. — C.	O.	C.	Diff. O. — C.
h. m.	h. m.	h. m.	m.	ft.	ft.	ft.
0 30	8 53	8 54	1	.75	.75	.00
1 30	8 38	8 42	4	.73	.72	.01
2 30	8 29	8 30	1	.64	.66	.02
3 30	8 24	8 24	0	.60	.58	.02
4 30	8 28	8 27	1	.48	.49	.01
5 30	8 42	8 40	2	.45	.43	.02
6 30	9 6	9 3	3	.42	.42	.00
7 30	9 23	9 23	0	.44	.46	.02
8 30	9 31	9 30	1	.53	.54	.01
9 30	9 28	9 28	0	.61	.62	.01
10 30	9 20	9 19	1	.68	.69	.01
11 30	9 9	9 7	2	.75	.74	.01
	8 57					

TABLE IV.

Second Six Months.

Moon's Age.	Interval.			Height.		
	O.	C.	O. — C.	O.	C.	O. — C.
h. m.	h. m.	h. m.	m.	ft.	ft.	ft.
0 30	8 53	8 52	1	.78	.77	.01
1 30	8 42	8 39	3	.72	.75	.03
2 30	8 27	8 28	1	.69	.68	.01
3 30	8 22	8 21	1	.59	.59	.00
4 30	8 22	8 23	1	.49	.49	.00
5 30	8 34	8 38	4	.46	.42	.04
6 30	9 4	9 3	1	.40	.40	.00
7 30	9 22	9 23	1	.44	.45	.01
8 30	9 31	9 31	0	.53	.54	.01
9 30	9 32	9 28	4	.62	.64	.02
10 30	9 20	9 19	1	.69	.72	.03
11 30	9 4	9 6	2	.71	.77	.06
	8 56					

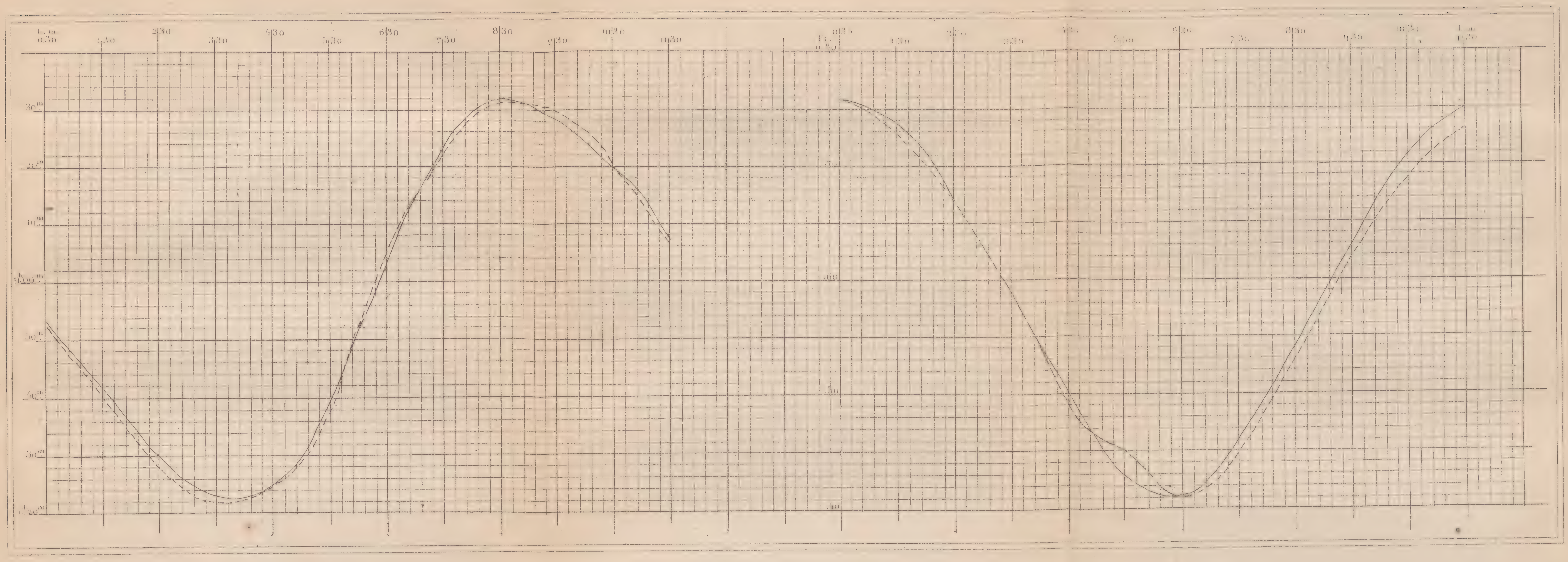
Plate No. 4

Comparison of Halfmonthly inequality in Interval and Height

from theory & observation

Semi-diurnal wave

No. 1 Interval *No. 2 Height*



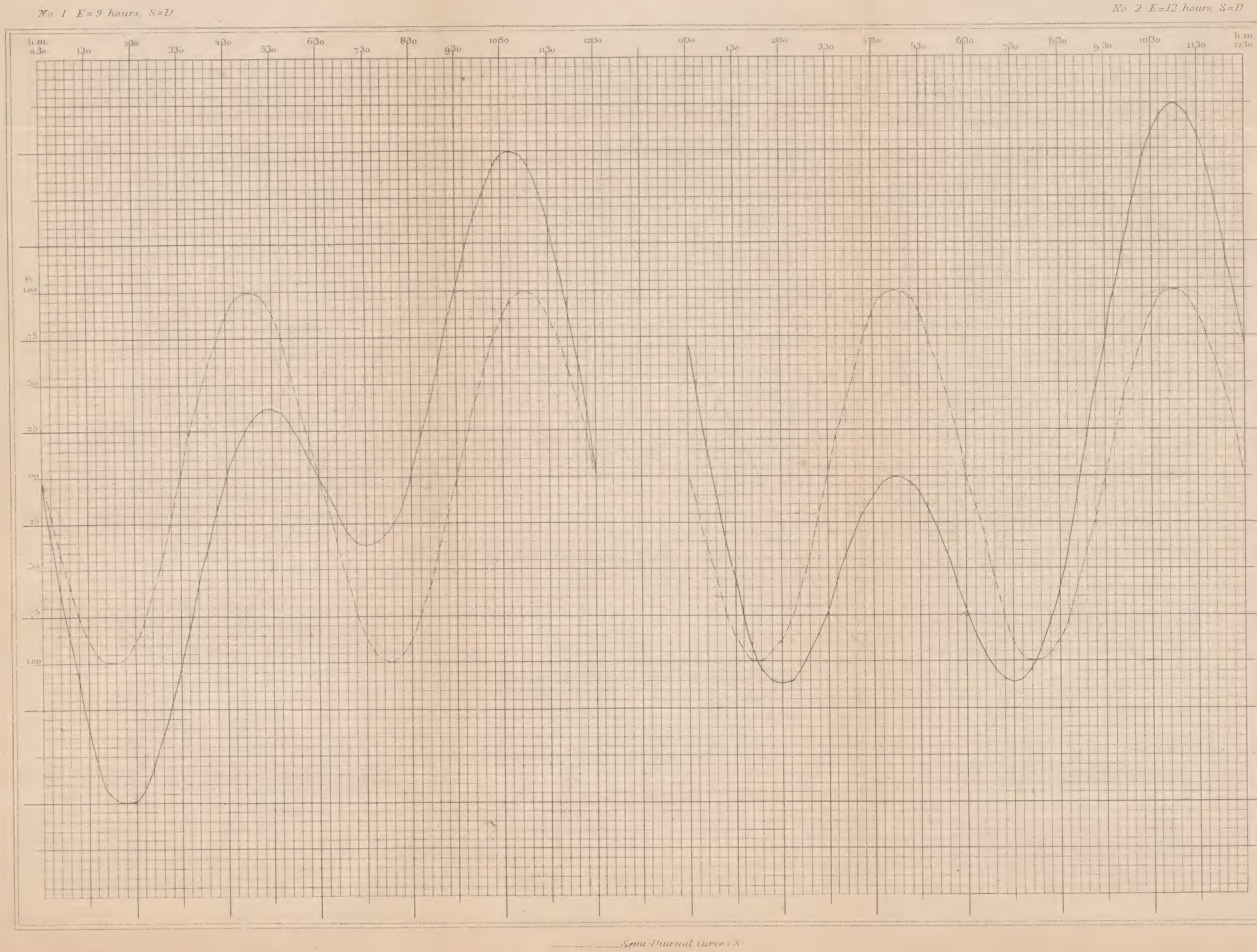
- - - - - - - - *Observation*

———————— *Theory*

Plate No. 5

Showing Change of compound curve with Change of E.

No. 1 E = 9 hours, S = D *No. 2 E = 12 hours, S = D*



- - - - - - - - *Semi-Diurnal Curve (S)*

Diurnal Curve (D)

———————— *Compound Curve*

TABLE V.

Whole Year.

Moon's Age.	Interval.			Height.		
	O.	C.	O. — C.	O.	C.	O. — C.
h. m.	h. m.	h. m.	m.	ft.	ft.	ft.
0 30	8 53	8 53	0	.76	.76	.00
1 30	8 40	8 40	0	.73	.74	.01
2 30	8 28	8 29	1	.67	.67	.00
3 30	8 22	8 22	0	.59	.59	.00
4 30	8 25	8 25	0	.49	.50	.01
5 30	8 38	8 39	1	.45	.43	.02
6 30	9 5	9 3	2	.41	.41	.00
7 30	9 22	9 23	1	.45	.46	.01
8 30	9 31	9 31	0	.53	.54	.01
9 30	9 30	9 28	2	.62	.63	.01
10 30	9 20	9 19	1	.69	.71	.02
11 30	9 6	9 7	1	.73	.75	.02
	8 57					

Curves showing the result of these comparisons are given in Plate IV. The greatest difference for the whole year, between the two sets of results, is but one minute of time for the interval, and 0.02 foot in the height.

The results are in apparent time, the substitution of which for mean time was, however, appreciable in but a slight degree.

There are several small corrections suggested by the hypothesis which has been adopted, but the small value of the residuals render the following of them up unnecessary. To the last computations we have reached no greater accuracy than is presented by these residuals, and could not safely base any conclusions on less quantities.

The ordinates used in the heights are the maximum ordinates of the component curves, and not those of high water of the compound curve; but it is easily shown that when the value of E, when most nearly constant, is, as at Key West, between about nine and nine and a half hours, this difference is inappreciable.

The solar day having been used in the decomposition instead of the lunar, the curves are at a mean twenty-five minutes behind their true place, and the mean luni-tidal interval differs twenty-five minutes from the truth. Adding this quantity, it agrees, as it should do, with the former determination.

For the reason just assigned, these numbers would require correction before using them to determine the constants. This, when made, gives the result as before stated.

Diurnal Tides.

The maximum ordinates found for the diurnal tides from the decomposition of the curves of observation were grouped according to the declinations of the moon, by magnitude without regard to sign, as shown in the first and second columns of Table VI.

The maximum ordinates may in this case be reduced to high-water ordinates, by a very simple process, and thus a comparison be established between this mode of reduction and the ordinary one. For $E = 9^{h.}\ 30^{m.}$, the high-water ordinate is 0.79 the maximum, provided, as in the case at Key West, the time of high water may be taken as that of the semi-diurnal wave.

The following table shows the sine of twice the moon's declination, the corresponding mean maximum ordinate, twice the high-water ordinate deduced from this (which is the diurnal inequality from our mode of reduction), the diurnal inequality as usually obtained, and the difference.

TABLE VI.

Sine twice Moon's Declination.	Maximum Ordinate.	Twice High-water Ordinate (C).	Diurnal Inequality (C').	Difference $C' - C$.
	ft.	ft.	ft.	ft.
11	.12	.20	.19	—.01
21	.17	.27	.28	.01
35	.28	.44	.44	.00
48	.38	.59	.60	.01
59	.46	.71	.70	—.01
66	.51	.80	.79	—.01
69	.54	.84	.83	—.01
	Mean,	.55	.55	

The results are represented in Plate No. 3.

The statement made above in relation to the high-water ordinates, is not true for those of low water, as the consideration of the formula $y = C \cdot \cos 2\,t + D \cos\,(t - E) - C$ will show, making $E > 9$ hours. The reverse is the case if $E < 9$ hours, the statement applying then to the inequality of low water, and not of high. At Key West, while the high-water inequality in height is thus readily found from the maximum ordinate, the low-water presents a less accordant result, while at Cedar Keys just the reverse occurs, as should be the case.

It is plain, also, that changes in the co-efficients C, D, and in E, will cause the inequalities in times and heights to vary, as well as those of

high and low water, losing all correspondence with each other, as is also well shown in the annexed diagram. Mr. Gordon suggests that in the value of E will be found the full explanation of the peculiarities of the Petropaulofsk tides described by the Rev. Mr. Whewell.

In diagram No. 1, Plate No. 5, E is assumed 9 hours, and $S = D$, and the inequalities of high and low water in interval and height correspond to each other. The same is the case for $E = 15$ hours. In No. 2, E is 12 hours, and $S = D$. The inequality in interval of H. W. is $0^{h.}$, of L. W. $4^{h.}$, when that in height of H. W. is 2 ft., and of L. W. 0 ft. For E 18 hours, and $S = D$, these inequalities would be reversed, that of the interval of H. W. being $4^{h.}$, and of L. W. $0^{h.}$, while for height the inequality of H. W. is 0 ft. and of L. W. 2 ft.

Using the high-water ordinates, determined as before stated, instead of the diurnal inequality in height, from which it has been shown not to differ sensibly, the numbers were compared with those of Mr. Lubbock's formula, $d\,h = B\,(A) \sin 2\,\delta \,.\, \cos(\psi - \phi) + \sin 2\,\delta' \,.\, \cos \psi$; neglecting the variations of $\cos(\psi - \phi)$ and $\cos \psi$, the co-efficients B and (A) B were found by least squares, for the separate six months and for the year, agreeing sensibly in the partial and total determinations. From two years' results $B = 0.56$, and $(A)\ B = 0.16$. The value of (A) thus obtained is, as it should be, the same as deduced from the half-monthly inequality.

The sum of the squares of the difference of the numbers from the formula and from the computed high-water ordinates is for the year but 0.0087 ft.; corrected for the moon's parallax, but 0.0078 ft.

TABLE VII.

Moon's Age.	Diurnal Inequality in Height. Observation — Theory.	
h. m.	Corrected.	Uncorrected.
0 30	.005	.010
1 30	.005	.005
2 30	—.035	—.030
3 30	—.035	—.035
4 30	—.075	—.080
5 30	—.070	—.070
6 30	—.080	—.085
7 30	—.090	—.090
8 30	—.065	—.065
9 30	—.020	—.020
10 30	—.030	—.030
11 30	.005	.010

The individual results are given in the foregoing table, in which the

first column contains the moon's age, the second the difference between the computed high-water ordinates and the corresponding quantities from the formula for the variations in the diurnal inequality in height corrected for parallax, and the third the same as uncorrected for parallax.

The residuals are very small, but follow the law of the half-monthly inequality as was found also from the corresponding results from the Cat Island observations.

The discussion of the value of E, which is in progress, I hope to present at a future meeting of the Association.

Changes of Mean Level.

The mean level of the water at Key West was seen from the observations to undergo remarkable changes, from one period of the year to another.

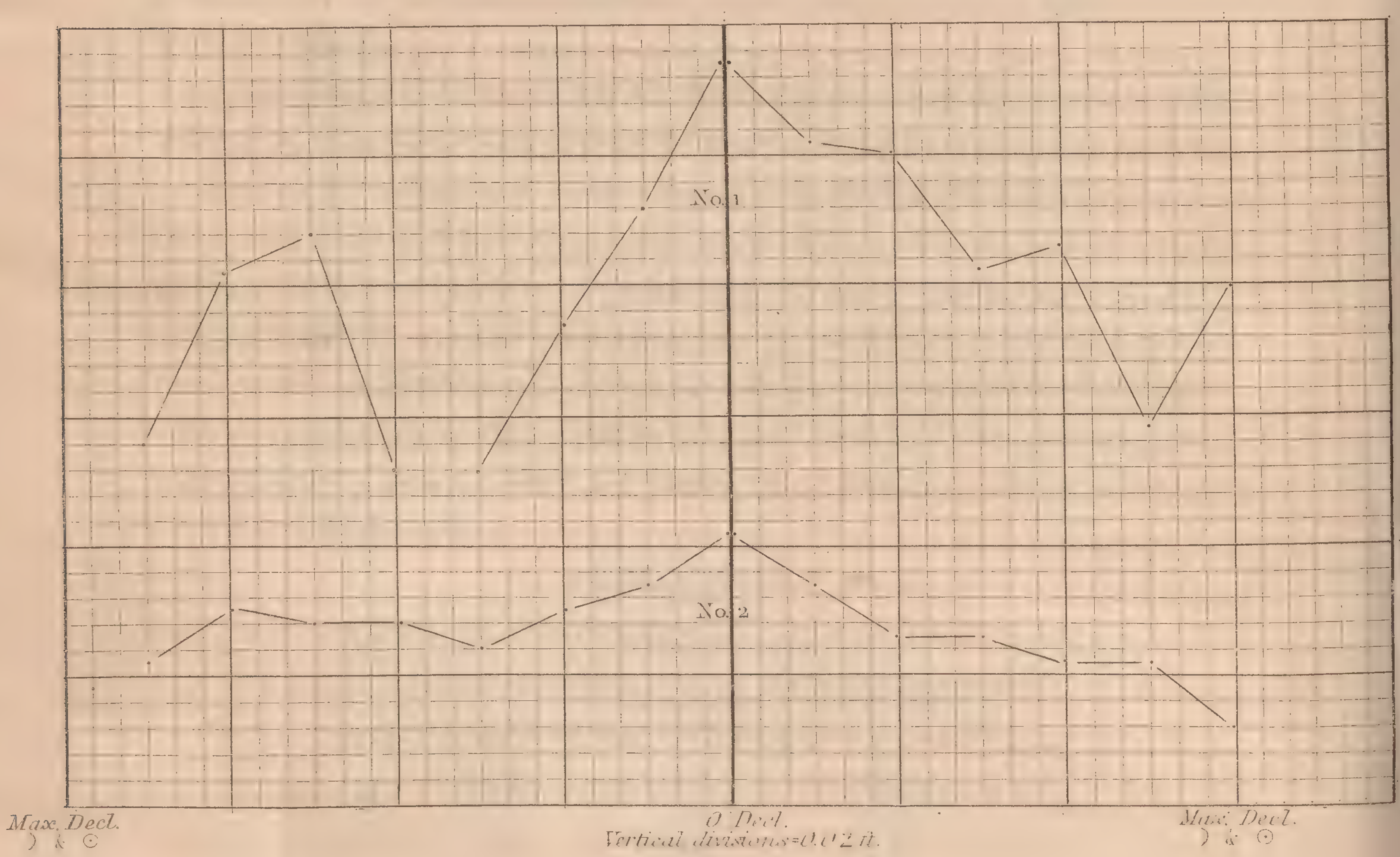
A comparison of the reductions for the first and second six months shows that the high water of neap tides of the first period rises actually to a higher level than the high water of the spring tides of the second. The mean level of the high water for the first six months exceeds that for the second by 0.48 ft. The form of the half-monthly inequality is perfectly regular in each six months; the gauge had remained undisturbed, and in seeking for the explanation it was observed that the mean level of the water varied very materially in the two periods, there being a change which appeared to go through its variations in the course of the year.

TABLE VIII.

Moon's Age.	Height of High Water.	
	1st Six Months.	2d Six Months.
h. m.	ft.	ft.
0 30	6.63	6.05
1 30	6.59	6.04
2 30	6.48	6.02
3 30	6.38	5.97
4 30	6.31	5.86
5 30	6.26	5.75
6 30	6.17	5.72
7 30	6.28	5.72
8 30	6.26	5.79
9 30	6.34	5.90
10 30	6.43	6.01
11 30	6.54	6.06
	6.39	5.91

Plate No. 6

Showing the mean level of the water at Key West the observations arranged according to the declination of the Moon (No.1) & the Sun (No.2) respectively.



The foregoing table shows the heights of high water at the several ages of the moon in the first and second six months, referred to the same zero.

I hardly supposed that the numbers representing these changes of level would furnish evidence of the two interesting tides of long period pointed out by Mr. Airy (Tides and Waves, *Encyc. Metrop.*, p. 355), but they do, and in the case of the moon's action, where the number of averages which can be brought to bear upon a single result is considerable, and the observations run through various parts of the year, the results bear carrying to numerical comparison with the formula. These tides, as far as I am aware, have not been developed from observation, though certain general analogies pointed to their existence. Dividing the numbers showing the daily level of the water into groups of nearly fourteen days, each corresponding to the moon's declination from the maximum to the maximum again, and taking the mean of each set corresponding to the same declination, we obtain a series which is the average of twenty-six numbers, in which the irregularities of the depressing and elevating action of the winds will be eliminated, and in which the sun's action will be nearly the same. This series presents a tolerable regularity, increasing to a maximum at zero of declination, as shown in the annexed Diagram 6, No. 1.

Taking the mean level of the water for each fourteen days, and dividing the results into two groups, corresponding to the same declination of the sun, north and south, we have a series of numbers which, though less regular than the others, also rise toward the zero of declination, as shown in Plate VI., No. 2.

The results of the first series of computations bear very well a comparison with the formula given by Mr. Airy,

$$-(1.34 \times \sin^2 \mu + 0.61 \times \sin^2 \delta)\ (\cos 2\lambda + c),$$

in which μ and δ are the declinations of the moon and sun respectively, and λ is the latitude of the place, requiring $c = 0$. Those of the second present greater discrepancies, and require $c = \frac{1}{3}$, contradicting the former. Though the weight of authority is that in favor of the criterion of the wave theory, $c = 0$, the result is inconclusive. In either case the whole number involved is less than the tenth of a foot, the theoretical lunar tide being 0.095, and the measured 0.098, while the theoretical solar tide for $c = 0$ is 0.046, and the measured is 0.077.

4 *

The regularity of the winds of this region (the trade-winds), taken in connection with the form of the harbor, points also to their action as a source of explanation of this change of level. The meteorological tables kept while the tidal observations were made furnish means of a complete discussion, which is in progress. I may remark now, that winds tending to elevate the water in the harbor prevail for six months, from March to August inclusive, and those tending to depress it, for the other six months, from September to February inclusive. The subject is one in which it is difficult to come to numerical results, because the variations in the force of the wind, and the duration, both enter into the effect, and distant action sometimes causes local effects. The whole rise and fall is nearly three quarters of a foot.

The mean level of the water, deduced from the mean of high and low water in each month, is shown in the annexed table.

TABLE IX.

Month.	Height in feet.	Month.	Height in feet.
June	5.60	December . .	5.31
July	5.78	January . . .	5.11
August . . .	5.63	February . . .	5.15
September . .	5.93	March	5.26
October . . .	5.90	April	5.26
November . .	5.73	May	5.32
Mean . . .	5.76	Mean . . .	5.24

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2. On the Tides on the Western Coast of the United States, from Observations at San Francisco, California, in Connection with the United States Coast Survey. By A. D. Bache, Superintendent. (Communicated by authority of the Treasury Department.)

Tidal observations have been made in connection with the hydrography of the Coast Survey at several points on the western coast, agreeing in showing the same interesting fact of the large diurnal inequality of the tides, already traced by Mr. Whewell in the observations at the Russian settlement of Sitka. The diurnal inequality

in height of the tides on the Atlantic coast is much more considerable than in Europe, and the diurnal inequality of interval is also well marked; but both require numerous carefully made observations to establish their laws, in consequence of the particular relation between the semi-diurnal and diurnal waves. On the Gulf of Mexico, west of St. George's Island, the semi-diurnal tide is almost merged in the diurnal, but the total rise and fall is quite small. At Key West, and along the western coast of Florida, where the diurnal inequality is large, the whole rise and fall of the tides is small, rendering numerous observations necessary to obtain reliable numerical results. The same is not the case on the western coast, observations made for a short period through the whole twenty-four hours showing a peculiarly large diurnal inequality as the most remarkable phenomenon of the tides. It becomes one of great practical importance to the navigator, for in San Francisco Bay a rock which has three and a half feet of water upon it at the morning high water, may be awash at high water of the afternoon, and charts on which the soundings are reduced to mean low water will have no accurate significance, being liable to an average error of the soundings at either low water of the day of 1.18 feet.

The results which I now present and propose to discuss are of two series of tides, observed in connection with the Coast Survey, at Rincon Point, in the city of San Francisco, California. The observations were under the direction of Lieutenant Commanding James Alden, U. S. Navy, one of the Assistants in the Coast Survey. They were made hourly, except about the times of high and low water, when the regular intervals were fifteen minutes, and the attempt was made to seize the precise time of high and low water. The first series extended from January 17th to February 15th, 1852, and the second from January 23d to February 17th, 1853. Another set of similar observations was made at Saucelito, on the northern side of the Bay of San Francisco, but not with the same care which appears to characterize these. The results are in the general accordant with those deduced from the Rincon Point series.

The reduction of the work of 1852 was made by Mr. W. W. Gordon, and that of 1853 by Messrs. Fairfield, Mitchell, and Heaton, of the tidal party at the Coast Survey Office.

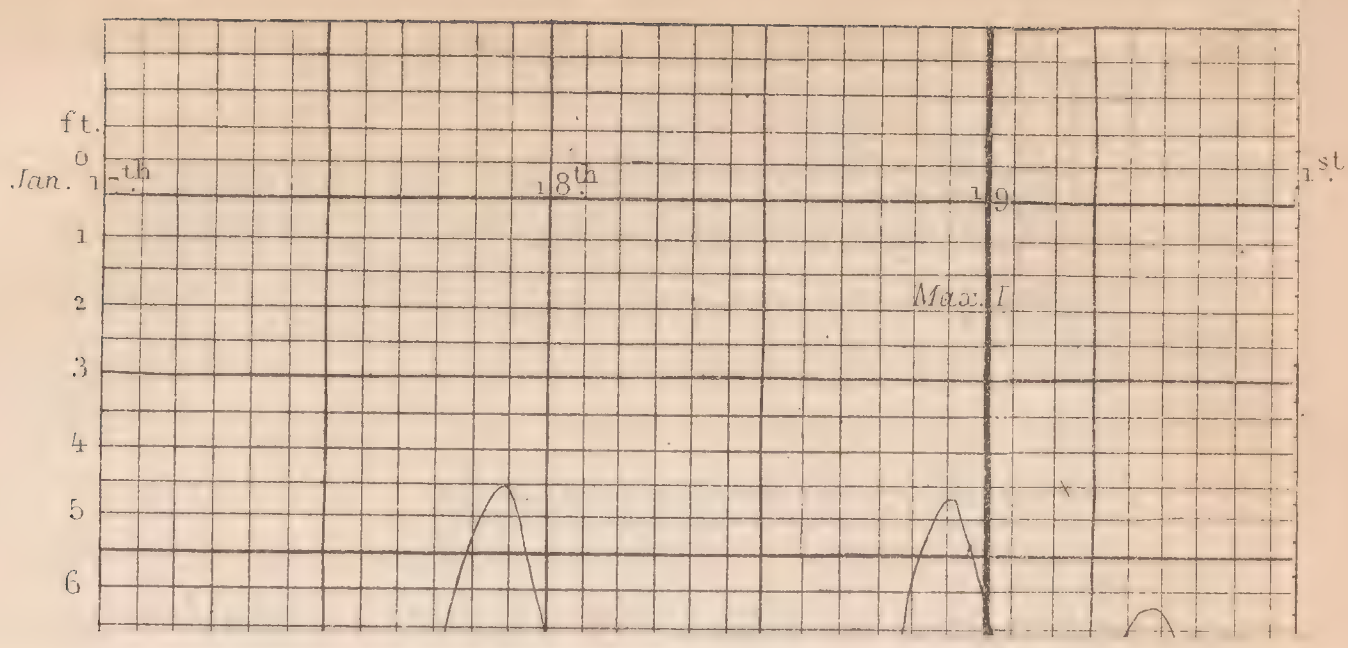
The results of 1852 are projected in the curves shown on Diagram

A, where the abscissæ represent the times from 0 hours, midnight, and the ordinates represent the heights. The scale is such that the intervals between the vertical lines correspond to two hours, and between the horizontal lines to half a foot. The curve begins with midnight of the calendar day, January 16th – 17th, and ends with noon of February 15th. The epochs of the moon's phases and of zero and maximum declination of the moon are marked at the head, and the times of transit at the foot, of the diagram, the curves upon which, for convenience of the page, have been divided into two parts, so arranged with respect to each other that the days of corresponding declination fall nearly over and under each other. The curves of the series of 1853 present the same general results, with about the same extent of irregularities.

These tides obviously present a case of large diurnal inequality in height, the interference of the diurnal and semi-diurnal waves going to produce one large and one small tide in the twenty-four lunar hours. When the declination of the moon is at its maximum, the difference in the heights of consecutive high and low waters is nearly at its maximum, and when the declination is nearly zero, the difference is the smallest.

The diurnal inequality in the interval is also perfectly well marked in these tides, amounting, when greatest, to about two hours for high water and one hour and eleven minutes for low water.

The usual discussions of the times and heights corresponding to the same time of transit of the moon were made from the two series of observations. A defect having been found in the operation of referring the level of one tide-gauge to the other, the two series of heights were combined, by assuming the mean height in each series to have been the same. The results were plotted on a diagram like B, but on a larger scale, for the purpose of graphical corrections in the mode used by Mr. Whewell. The ordinates of the diagrams, Nos. 1 and 2 (Diagram B), correspond to the luni-tidal intervals, and of Nos. 3 and 4 to the heights; the abscissæ in each case, to the hours of the moon's transit. The scale is shown at the top and side of each diagram. No. 1, Diagram B, shows the results for the half-monthly inequality of interval of high water and the curves traced by them, No. 2 the same for low water; No. 3 shows the half-monthly inequality in the height of high water, and No. 4 in that of low water; the dots show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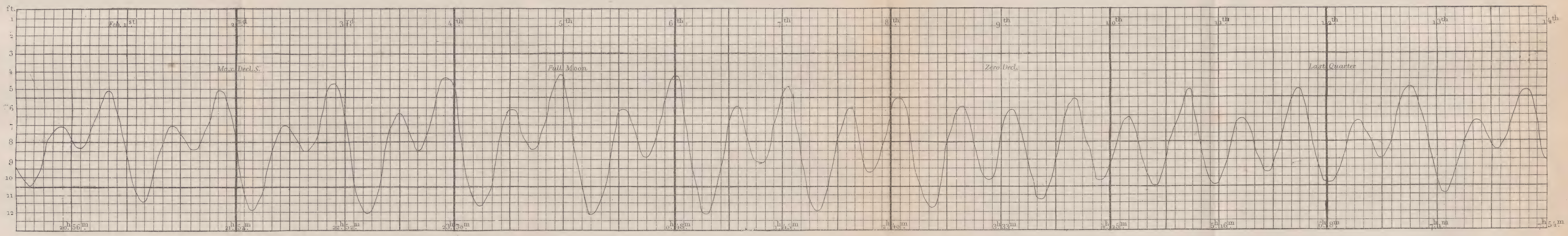
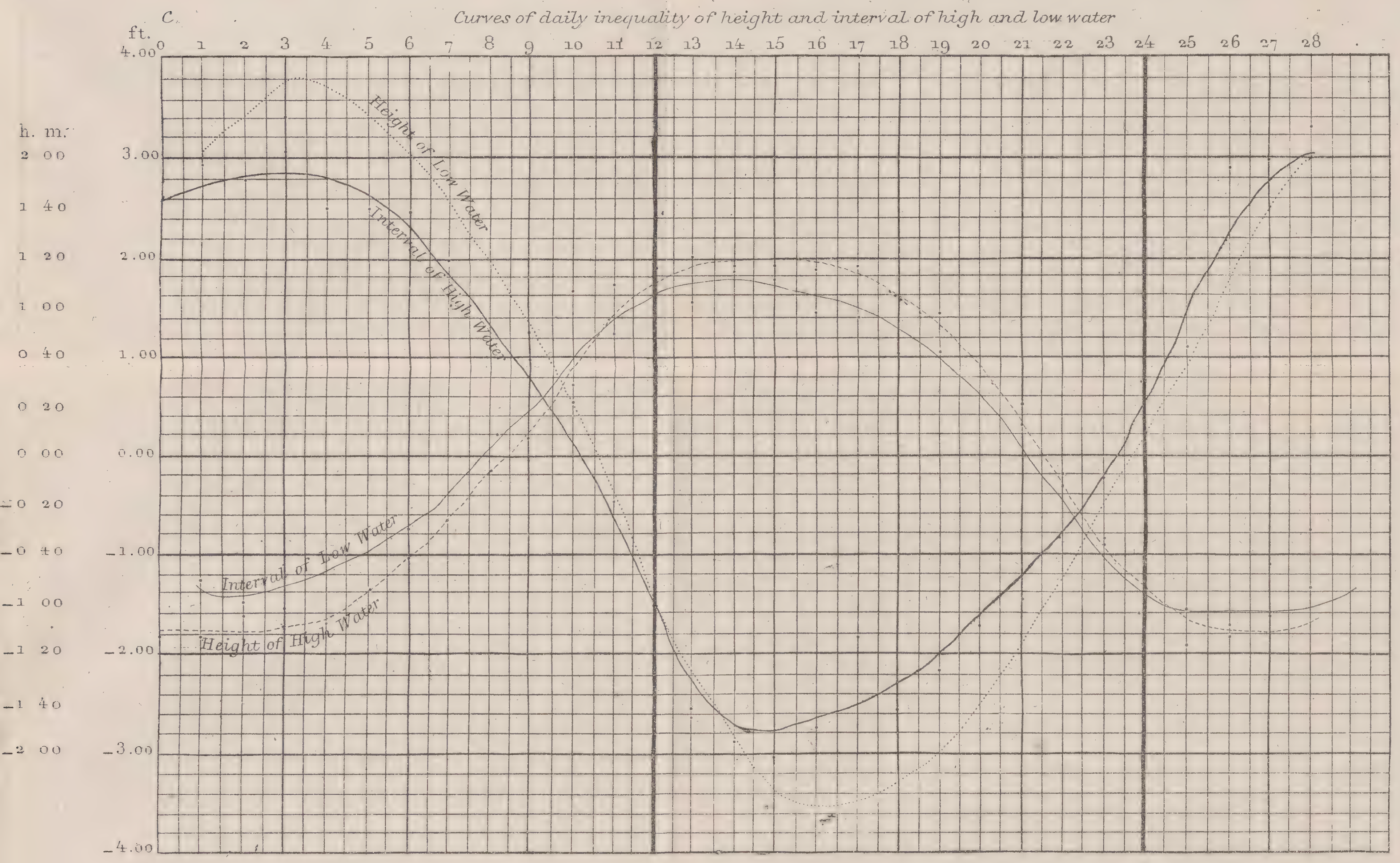
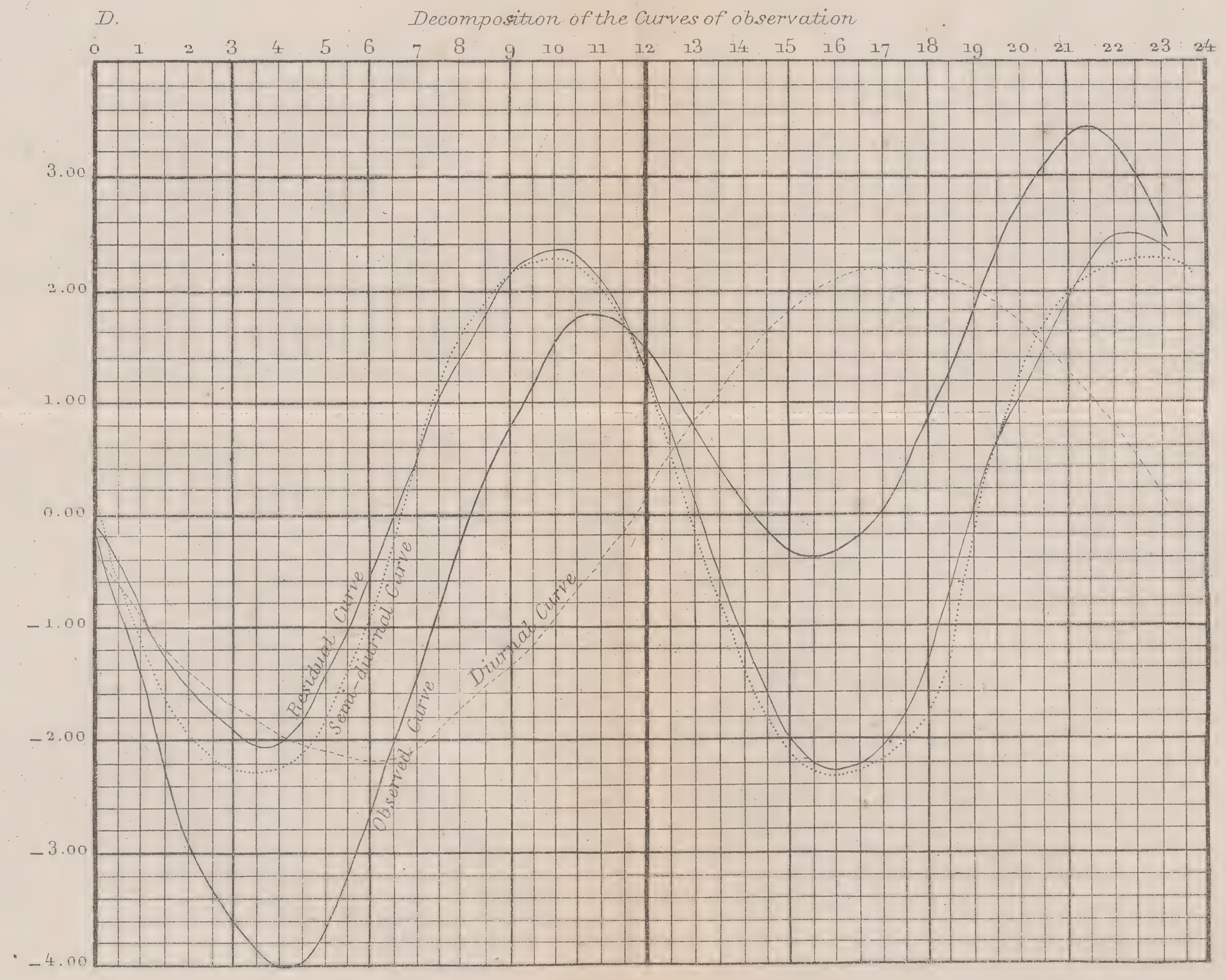
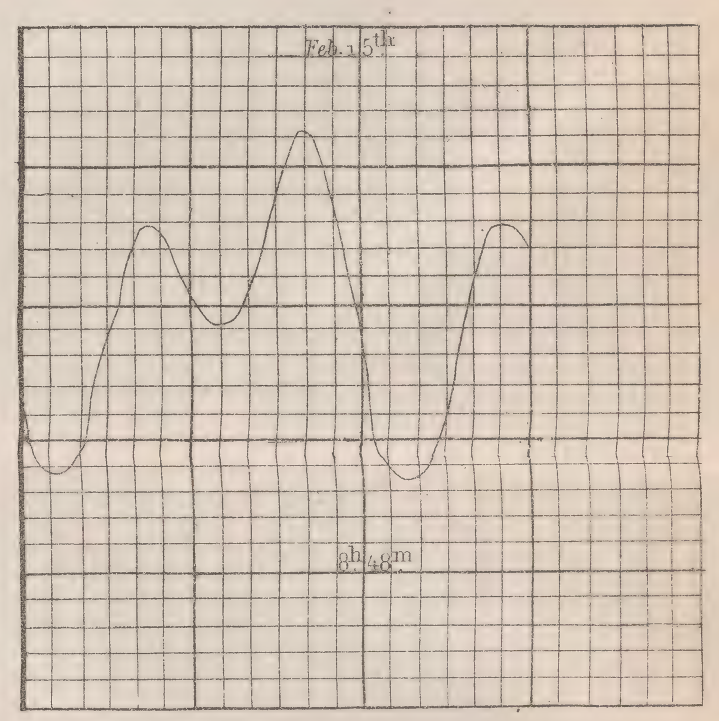
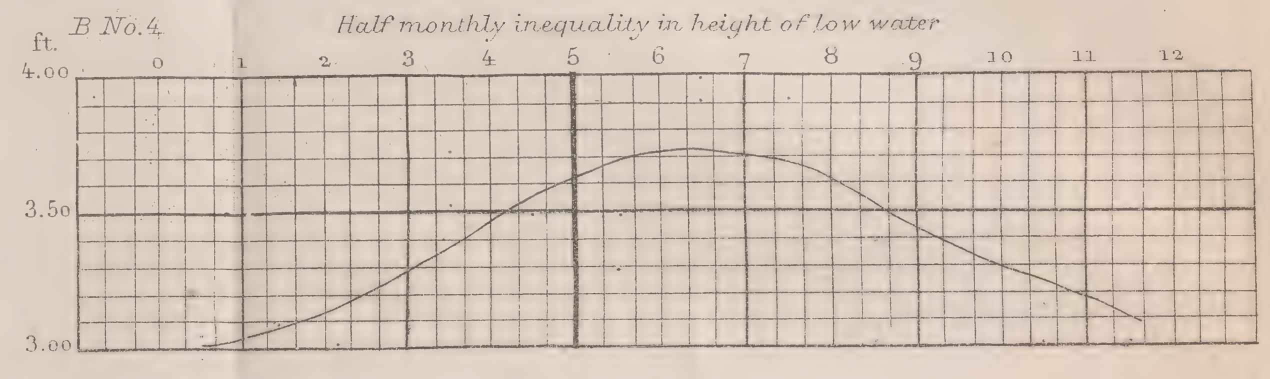
Curves representing the hourly observations of the Tides

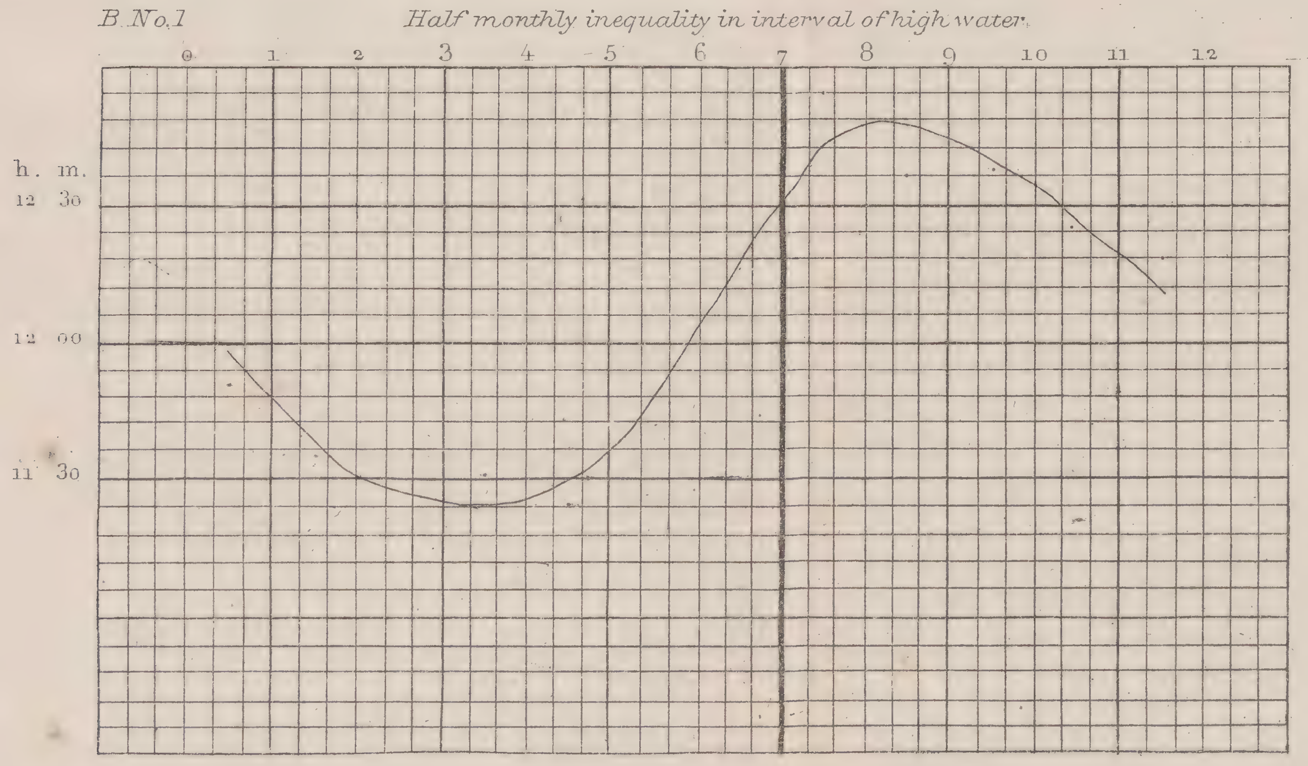


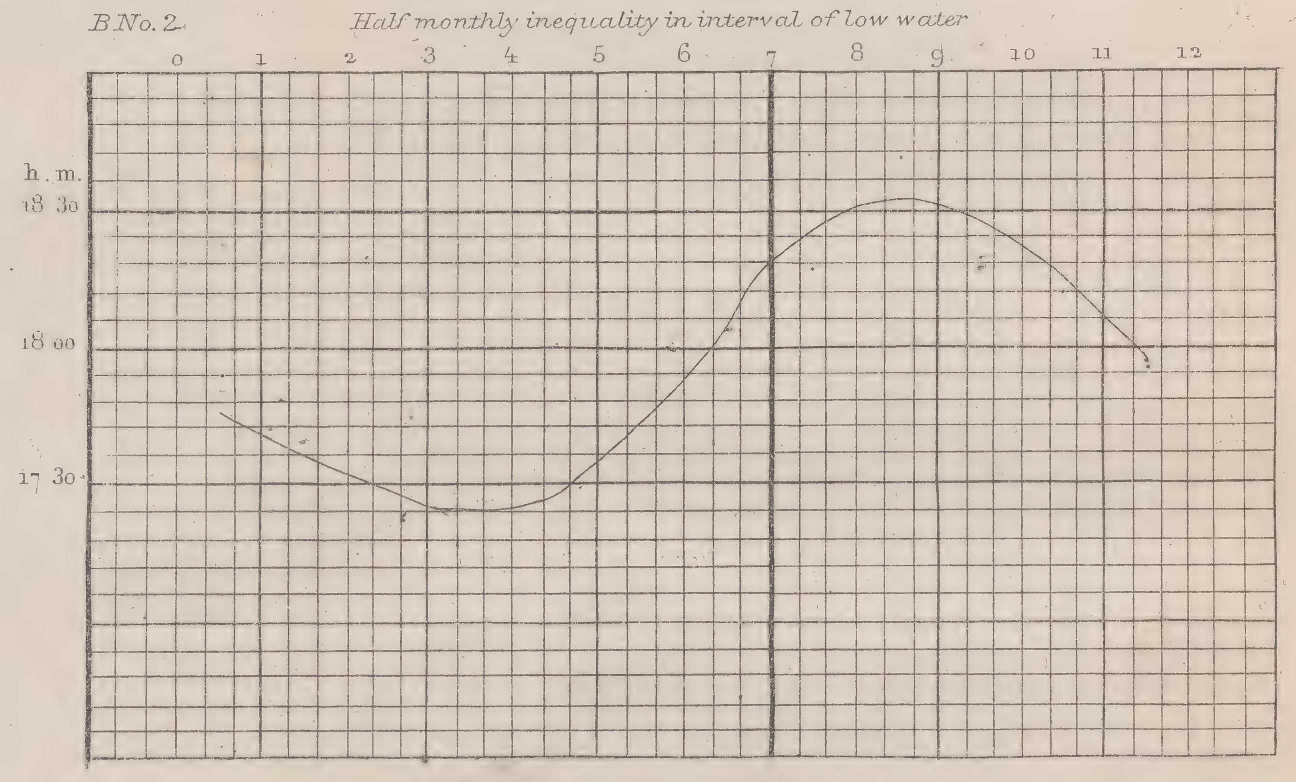


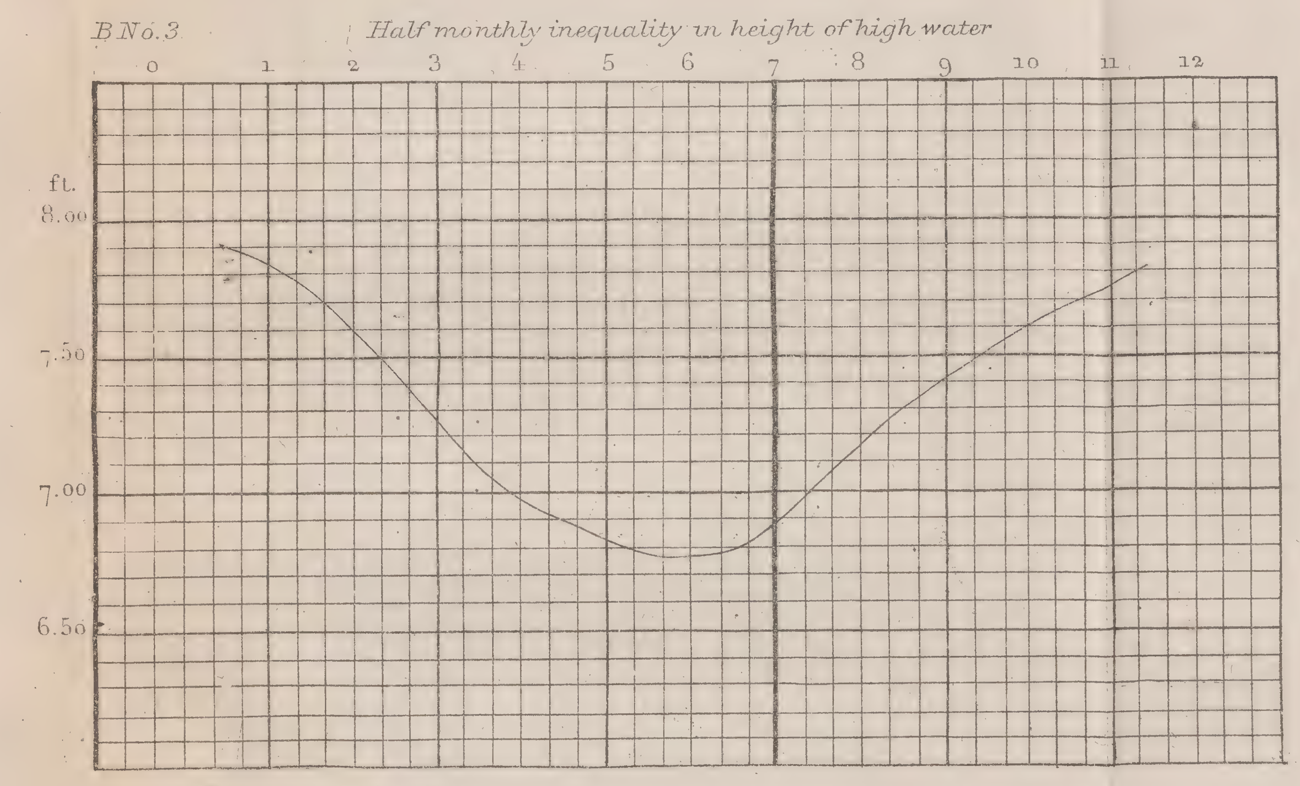












U. S. COAST SURVEY

A.D. Bache Superintendt.

Tides at Rincon Point San Francisco

CALIFORNIA

Jan. & Feb.

1853

where the observations fall. The comparison of the curves with the observations is given in the annexed table.

TABLE I.

Comparison of Approximate Curves of Half-monthly Inequality of the Tides at Rincon Point with Observations.

Moon's Age.	Interval.				Height.				Moon's Age.
	High Water.		Low Water.		High Water.		Low Water.		
Transit F.	From Curve.	Obser'n — Curve.	From Curve.	Obser'n — Curve.	From Curve.	Obser'n — Curve.	From Curve.	Obser'n — Curve.	Transit F.
h. m.	h. m.	m.	h. m.	m.	ft.	ft.	ft.	ft.	h. m.
0 30	11 59	—10	17 45	— 9	7.90	— 3	3.02	+ 3	0 30
1 30	36	— 2	37	— 3	7.75	+ 4	3.10	— 8	1 30
2 30	27	0	28	1	7.45	+ 2	3.20	—20	2 30
3 30	24	7	24	7	7.10	+ 2	3.38	+29	3 30
4 30	28	— 4	28	— 9	6.90	— 4	3.52	— 6	4 30
5 30	48	— 7	42	18	6.78	+12	3.68	—29	5 30
6 30	12 18	— 1	18 5	— 1	6.80	—13	3.70	+26	6 30
7 30	43	12	24	— 7	7.00	0	3.67	—18	7 30
8 30	46	—12	33	9	7.30	+15	3.50	—41	8 30
9 30	39	— 2	27	— 6	7.50	—15	3.40	+27	9 30
10 30	27	— 1	15	— 3	7.69	+ 5	3.25	—19	10 30
11 30	11	8	17 57	— 2	7.80	— 4	3.10	+ 6	11 30
Mean,	12 3	+27 —39	17 55	+35 —40		+39 —40		+110 —106	

The results, both for interval and height, are very good, considering the small number of observations (four) of which each is the mean; the heights are, as usual, less regular than the times, and the results for the inequality of the height of low water are the least regular of all.

The approximate mean luni-tidal interval for high water, or corrected establishment of Rincon Point, is $12^{h.}\ 3^{m.}$. This corresponds to an epoch of 0 hours, showing that the tides belong to the next preceding transit (transit F) of the moon, and not the fifth preceding (transit B), as was found by Mr. Lubbock, for the tides of Great Britain. The epoch for low water corresponds also almost exactly to 0 hours. The same thing is shown, less forcibly however, by the discussion of the observations before referred to at Saucelito.

From curve No. 1 it appears that the difference in the luni-tidal intervals for $3^{h.}$ and for $9^{h.}$ is $1^{h.}\ 20^{m.}$, or (A) of Mr. Lubbock (tan 20°) is 0.342. The difference between the heights of high water at spring and neap tide is, from Diagram No. 3, 1.12 ft., and (E) of Mr. Lubbock, $\frac{1.12}{2\,(A)} = 1.66$. The two series of observations, discussed separately,

gave results which did not differ materially from these. These numbers will serve as a first approximation.

It should not be forgotten, that the observations having been made in successive years, in the same month, the moon's age and declination and the sun's declination are not very different, and the sun's declination is nearly the same on the corresponding days.

The *diurnal inequality* obtained by the usual method is given in the annexed table (No. 2). The two series are combined by taking the averages for the days on which the declinations correspond in the two series; each average is thus the mean of four individual results.

TABLE II.

Diurnal Inequality of Interval and Height for High and Low Water, from Observations in January, 1852 *and* 1853, *at Rincon Point, San Francisco, California.*

High Water.		Low Water.		
Interval.	Heights.	Interval.	Heights.	
h. m.	ft.	h. m.	ft.	
1 44	—1.85			Jan. 19, 1852, and
1 57	—1.81	—0 51	3.01	Jan. 25, 1853.
1 47	—1.63	—1 3	3.44	
2 17	—1.59	—0 47	3.75	Moon's max. dec. S.
1 41	—1.62	—0 40	3.72	
1 43	—1.33	—0 40	3.46	
1 41	—1.05	—0 30	3.07	
1 20	—0.66	—0 23	2.57	
0 39	—0.17	+0 7	2.01	
0 52	0.32	0 38	1.37	Moon's dec. zero.
0 23	0.71	1 5	0.50	
—0 18	1.72	0 50	—0.41	
—1 1	1.90	0 58	—1.44	
—1 42	2.01	1 1	—2.19	Feb. 1, 1852, and
—1 55	1.88	1 14	—2.85	Feb. 3, 1853.
—2 1	1.86	1 12	—3.36	
—1 49	1.84	1 0	—3.48	Moon's dec. max. N.
—1 12	1.90	0 57	—3.48	
—1 41	1.65	0 41	—3.28	
—1 26	1.42	0 42	—2.99	
—1 9	1.00	0 35	—2.50	
—0 59	0.29	0 20	—1.90	
—0 32	—0.30	—0 8	1.29	Moon's dec. zero.
—0 9	—0.97	—0 35	—0.50	
0 29	—1.42	—1 15	0.26	
0 46	—1.56	—1 17	0.95	
1 51	—1.70	—1 12	1.85	
1 58	—1.62	—0 46	2.51	Feb. 15, 1852, and
2 11	—1.35	—0 30	2.99	Feb. 17, 1853.
Mean, 1 21	1.36	0 47	2.36	

These numbers are projected on Diagram C, where the ordinates correspond to the intervals for one curve, and to the heights for the other, and the abscissæ to the tidal days for both. Notwithstanding the small number of observations, the curves can be traced with tolerable certainty, and follow the general law of the inequalities.

Each curve shows an inequality increasing and decreasing with the moon's declination nearly, crossing the zero line at or near the zero of declination, and reaching a maximum or minimum at the maximum of north or south declination. The observations do not furnish sufficient evidence to decide positively that the epochs of the several inequalities coincide with those of the declination, or otherwise. On the average, they are about half a day defore the corresponding declination.

The inequalities in the height of high water and in the interval of low water increase and decrease together, and so of the inequality of high water and height of low water.

The declination of the moon, and the inequality in interval of low water and in height of high water, have the same sign; the reverse is the case with the other two inequalities. The inequality in the height of low water is, in general, greater than in that of high water, exceeding it, when at the maximum, in the proportion of two to one nearly (1.9 to 1). The same relation exists between the maximum inequality in interval of high water as compared with that of low (1.7 to 1).

The maximum inequality in the height of low water is 3.60 ft., and of high-water 1.85 ft. The maximum inequality of interval of high water, as shown by the curve, is $1^{h.}\ 53^{m.}$, and of low water, $1^{h.}\ 7^{m.}$.

I am indebted to Mr. Heaton, of the tidal party, for the decomposition, under the direction of Mr. W. W. Gordon, of the curves of the daily observations in 1852, by the method adopted by me for the discussion of the tides of the Gulf of Mexico. Though, from some trials which I have made, these decompositions may be improved, they are nevertheless of great interest, and show well the causes of the forms assumed by the curves of diurnal inequality in height and interval, and for high and low water, and their relations. When the observations now in progress on the western coast shall have given additional results, I propose to take up this branch of the subject again. In the mean time, it appears to me the results now obtained are of sufficient interest to be presented to the Association.

I have taken, as an example of the decomposition, the curve from the observations of January 21st, 1852, the results corresponding nearly to the maximum of the moon's declination, and to full moon.

The diurnal curve, the interference of which with the semi-diurnal, produces the form shown in Diagram A, and also, on a larger scale, in Diagram D, is given on the diagram. Its maximum ordinate, as found by summing the two series of heights from the hourly observations, in which the same values of the ordinate of the diurnal curve occur with opposite signs, and referring to the curve of sines for their relation to the maximum ordinate, is 2.20 feet. The sum of the squares of the differences between observation and computation is the least when the interference takes place as shown in the Diagram D, the maximum ordinate of the diurnal curve being seven hours and a half from the maximum ordinate of the semi-diurnal curve. Subtracting the ordinates of the diurnal curve, assumed as a curve of sines, from the heights given by the hourly observations, we have a residual curve, which is traced on the diagram. The average of the four loops of this curve is almost precisely a curve of sines, of which the maximum ordinate is 2.30 ft.

The tidal curves near the maximum of declination, and for several days each side of it, result from the interference of a semi-diurnal and diurnal wave, which, at the maximum of each, are nearly equal in magnitude, the crest of the diurnal wave being, at that period, about eight hours in advance of that of the semi-diurnal wave.

The annexed table gives the comparison made in the diagram. The first column contains the ordinates of the curve of observation. The second contains those of the diurnal curve of sines. The third contains those of the residual curve. The fourth contains the ordinates of the semi-diurnal curve, which most nearly satisfy the residual. The fifth contains the small remaining differences, on the average being about 0.14 ft. The crest of the diurnal curve is seven and a half hours from the semi-diurnal, and its maximum ordinate is 2.2 ft.

TABLE III.

Analysis of Curve of Observations for January 21, 1852. — Rincon Point.

Ordinates. Curve of Observations.	Ordinates. Diurnal Curve of Sines.	Ordinates. Residual Curve.	Ordinates. Semi-Diurnal Curve of Sines.	Differences.
ft.	ft.	ft.	ft.	ft.
—0.23	—0.28	0.05	0.00	0.05
—1.63	—0.83	—0.80	—1.10	0.30
—2.98	—1.33	—1.65	—1.82	0.17
—3.63	—1.72	—1.91	—2.27	0.36
—4.03	—2.00	—2.03	—2.26	0.17
—3.68	—2.16	—1.52	—1.70	0.18
—2.73	—2.16	—0.57	—0.70	0.13
—1.48	—2.00	0.52	0.70	—0.18
—0.23	—1.72	1.49	1.65	—0.16
0.77	—1.38	2.15	2.20	—0.05
1.47	—0.83	2.30	2.30	0.00
1.72	—0.28	2.00	1.90	0.10
1.52	0.28	1.24	1.60	—0.36
0.77	0.83	—0.06	0.00	—0.06
0.17	1.33	—1.16	—1.30	0.14
—0.33	1.72	—2.05	—2.05	0.00
—0.28	2.00	—2.28	—2.28	0.00
0.07	2.16	—2.09	—2.15	0.06
0.87	2.16	—1.29	—1.50	0.21
1.87	2.00	—0.13	—0.20	0.07
2.72	1.72	1.00	1.20	—0.20
3 32	1.33	1.99	1.97	0.02
3.27	0.83	2.44	2.32	0.12
2.62	0.28	2.34	2.20	0.14

For equal maximum ordinates of the diurnal curve and semi-diurnal curve 2.1 ft., we have, for $E = 8$ hours, the diurnal inequality in height of high water, 2.03 ft., or .18 ft. greater than the mean found by the curve of diurnal inequality, and of low water, 3.57, or .03 ft. less than the value given by the curve. So, also, for the inequality in the intervals of high and low water, we have respectively 105 and 61 minutes, instead of 113 and 66, given by the diagram, differing but 8 and 5 minutes respectively, and having the same ratio to each other as the latter numbers. The mode of interference thus explains satisfactorily the curious relations of the inequalities of both time and height of high and low water.

Taking the values of the maximum ordinate of the diurnal curve (D), as deduced by Mr. Heaton, tracing a curve for them, and folding this over on its greatest ordinate as a hinge, we bring five values of D to the determination of each point in the curve, from the observations of 1852. Treating the curve of twice the sine of the moon's declination in the same way, we obtain a curve for comparison with

the former. Neglecting the sun's action, we have for theory, $m \sin 2\,\delta' = D$. Taking the mean of the values of D, which nearly correspond to each other in the half-declination period, and the mean of the corresponding values of the sine of twice the declination, we obtain $m = 29$, nearly.

The following table (No. IV.) gives a comparison of the values of the semi-diurnal ordinates, and of $m \sin 2\,\delta'$.

I have also deduced the diurnal inequality from Mr. Heaton's compound or interference curves, and have compared it in the same way with $m \sin 2\,\delta'$. The value of m found from these was $m = 28$. The last column of Table IV. refers to this comparison.

TABLE IV.

Showing the Values of the Maximum Ordinate of the Diurnal Curve (D), deduced from analyzing the Curves of Observation and Comparison with Theory. Also the Value of Comparison of the Diurnal Inequalities, measured on the Compound Curves.

No.	D.	$m \sin 2\,\delta'$.	Difference.	Diurn. Ineq. Height H. W. from Curve.
	ft.	ft.		ft.
1	2.13	2.17	.04	2.2
2	2.01	2.00	—.01	2.05
3	1.79	1.83	.04	1.85
4	1.54	1.55	.01	1.5
5	1.16	1.20	.04	1.1
6	0.81	0.76	—.05	1.6
7	0.25	0.26	.01	0.0

The agreement of the several results compared appears very satisfactory.

The changes in the value of E have been distinctly traced by Mr. Heaton; from the observations but before presenting the conclusion on this subject, I desire to subject them to the test of further computations, which are now in progress.

In order not to interfere with the regular work of the hydrographic party, a separate tidal party has been organized, under the direction of Lieutenant Trowbridge of the Corps of Engineers, Assistant in the Coast Survey, and supplied with the necessary means for a full investigation of the tides of our western coast. It is proposed to establish three permanent self-registering tide-gauges, under intelligent supervision, at San Diego, San Francisco, and Columbia River entrance, and to connect them by observations at suitable intermediate points. There are difficulties to be overcome in the character of the coast itself,

and of the aborigines who still inhabit portions of it; but I expect, nevertheless, entire success from the zeal and ability of Lieutenant Trowbridge.

The following tide-table results from the observations already discussed: —

		h.	m.
Corrected establishment at Rincon Point.	High Water . .	12	3
" " "	Low Water . .	17	51

	ft.	in.
Mean rise and fall of tides	3	11
" " of spring tides	4	11.8
" " of neap tides	2	11

Mean duration of rise, 6 hours 30 minutes, including half the stand.
" " fall, 5 " 52 " " " "
" " stand, 30 " " " "

Diurnal inequality in Height.	High Water.		Low Water.	
	ft.	in.	ft.	in.
Average for the whole month . . .	1	3	2	4
Greatest value	2	10	3	6½
Diurnal Inequality in Interval.	h.	m.	h.	m.
Average for the whole month . . .	1	2	0	45
Greatest value	2	0	1	6

Difference in height of highest high tide and lowest low tide in day.

	ft.	in.
Average	5	11
Greatest	7	7

When the moon's declination is North, the highest of the two high tides of the day is the one which occurs about twelve hours after upper culmination.

I have given elsewhere, for the use of navigators, a set of rules founded on these observations, and containing no technical terms not familiar to them.

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3. On the Rising of Water in Springs immediately before Rain. By Professor John Brocklesby, of Trinity College, Hartford.

My attention was particularly called to this phenomenon during the close of the summer of 1852, while residing for a few weeks in

Rutland, amid the highlands of Vermont. In the western portion of the town is a lofty hill, rising to the height of about four hundred feet above the Otter Creek valley. Near the summit of the hill a small spring bursts forth, the waters of which are conveyed in wooden pipes to the barn-yards of two farm-houses situated on the slope of the hill; the first being about a quarter of a mile distant from the spring, and the second nearly one third of a mile. At the latter house I resided. The waters of the spring are not abundant, and during the summer months frequently fail to supply the aqueduct.

Such was the state of the spring when I arrived at Rutland; for the summer had been extremely dry, the brooks were unusually low, and the drought had prevailed so long, that even the famed Green Mountains had, in many places, begun to wear a russet livery. The drought continued, not a drop of rain falling, when one morning a servant, coming in from the barn-yard, affirmed that we should soon have rain, as the water was now flowing in the aqueduct, the spring having risen several inches. The prediction was verified, for within two or three days rain fell to a considerable depth.

In a short time the spring again sunk low, and ceased to supply the aqueduct; but one cloudless morning, when there were no visible indications of rain, its waters once more rose, flowing through the entire length of the aqueduct, and ere twenty-four hours had elapsed, another rain was pouring down upon the hills. On inquiry, it was ascertained from the residents in the vicinity, that this phenomenon was one of ordinary occurrence, and that for the last twenty years the approach of rain was expected to be indicated by the rising of the spring.

Interested by these facts, I sought for others of a like nature, and requested through the public prints information upon this subject from all who happened to possess it, and also upon collateral points which were conceived to have important relations to this phenomenon. I was rewarded by the knowledge of only one additional instance, existing in Concord, Massachusetts, where a spring that supplies a certain brook is said to rise perceptibly before a storm. Mr. William Munroe, who lives near the stream, kindly offered the following information, which is given below in nearly his own words.

"Although I have frequently been informed that the 'Dodge's Brook' (so called), after a dry time, and when no water had run for

some days, would begin again to run when the clouds threatened rain, but before a drop had fallen, yet I cannot say that I have ever taken much pains to investigate the fact. However, I perfectly recollect being at one time near the brook when there had been a long drought. The clouds threatened rain very soon, — not a drop of water had run in the bed of the brook for some days, — not a drop had fallen from the clouds, and no rain had occurred in the vicinity. The course of the brook is across the road; I was standing in the road watching the brook, and then I saw a small stream of water in its bed flowing towards the river, which is about fifteen rods distant from the road. The spring that supplies the brook is situated about half a mile from the river, and is sometimes so powerful that I have known the brook to overflow the road for the space of several rods.

"I cannot say that it is an established fact that the brook *always* rises before a rain, but I have good reason to suppose that it does."

The preceding statements in respect to Dodge's Brook are corroborated by the son of Mr. Munroe, who writes thus: —

"The subject has not, so far as we are aware, fallen under the notice of any close observer of the facts you inquire about; the most that is known being this, that the bed of the brook, during a long drought, having become dry, the *stream is known to start again before a rain*, and the belief is that *rain is to be looked for immediately upon the reappearance of Dodge's Brook.*"

The cause of this phenomenon has been attributed by some to the fall of rain at the distant sources of the spring, a short time previous to its descent in the vicinity of the spring itself; but this view is, doubtless, erroneous, since it is altogether improbable that rains should fall at two distinct localities, year after year, with the same constant period of time between them, and that this interval should be such as to insure that water falling at the first locality should *always* arrive, through subterranean channels, at the second before the rain there commenced.

I have not been able to ascertain the state of the barometer, either at Rutland or at Concord, at the times when the phenomenon in question occurs; nevertheless, I believe the true solution will be found in the *diminished atmospheric pressure which exists before a rain.*

The waters of a spring remain at any given level, because the atmospheric and hydrostatic pressures combined exactly counter-

balance the upward force of the jet. The spring will therefore rise, either when the force of the jet is increased while the atmospheric pressure continues the same, or when the latter is diminished while the former remains constant; and the elevation is greatest of all when the decrease in the density of the atmosphere occurs simultaneously with an increase in the strength of the jet.

The rising of the water in the instances related cannot, I think, in view of the facts detailed, be fairly attributed to any sudden augmentation of force in the current of the springs; but is to be regarded as the result of diminished atmospheric pressure, occurring at these particular times in perfect accordance with known meteorological laws. I am not aware that it has yet been ascertained whether this phenomenon is local or general. If the latter should be found true, and the explanation given correct, we arrive at the curious discovery, that the springs and fountains of the earth are *natural barometers*, whose indications may perhaps be worthy of notice in future physical investigations.

IV. METEOROLOGY.

1. On the Value of the Barometer in navigating the American Lakes. By W. C. Redfield, of New York.

Mr. Redfield commenced by alluding to the great American lakes as remarkable, not more for their extent and commercial advantages, than for the destructive storms with which they are visited; these storms being sudden, and the shores generally dangerous. The rapid onset of these gales, when blowing from the westward, is in part due to the geographical position; and the warnings first afforded by the fall of the barometer under the preceding and lighter winds from other points of the horizon, have been too often unheeded. For the force of the easterly gales of the Atlantic is seldom felt on the lakes.

Each storm should be viewed as a great cyclone, or revolving movement of the wind, which has at the same time a constant motion of progression. In our latitudes, the first winds of the cyclone are

from an easterly or southerly quarter, preceded and attended by a fall of the barometer, which phenomena are due to the northeasterly progress of the cyclone, and its constant revolution from right to left around its own axis. On the Atlantic coast, these early winds of the storm commonly act in their full force. But not so in the interior, where their force is often broken by the intermediate elevations and obstructions. These also serve, in their turn, to protect the coast region, in some degree, from the sweeping violence of the colder winds which belong to the later portions of the passing cyclone.

On the great interior plateau where the lakes are situated, these conditions are partially reversed. The easterly or first winds of the cyclone, instead of a fair sweep over open sea, are impeded by the unequal surfaces of the country before reaching the lakes, and are less violent than the succeeding westerly winds. In all cases, when the axis of the cyclone is passing the observer, the barometer has fallen to its lowest point. Its rise now commences, and the wind becomes westerly, often with a suddenly increased force. These violent westerly or northwesterly winds are doubtless aided by the subsidence of the rapid and colder overlying current, which thus imparts to the later winds of the cyclone an almost irresistible force, as they sweep unimpeded over the surface of the great lakes.

We have seen that, in the North American cyclones, the first or advanced portion exhibits easterly or southerly winds, the particular direction, locally considered, being dependent upon the position of the observer laterally, in the cyclonic path. As these first winds exhibit a fall of the barometer, the progress and degree of this depression demands careful attention from the navigator, especially on the lakes. For the force and duration of the westerly gale which constitutes the rear of the cyclone will be proportioned to the fall which has occurred. The navigator should remember, that when the barometer ceases to fall, the centre wind is nearest; the local change of wind will soon follow, and the barometer will commence rising; and that this is the most dangerous period of the storm, of which the barometer has thus given us indication. All precautionary measures should be taken, then, during the fall of the barometer. If by the favorable direction of the easterly or first winds of the cyclone, navigators are tempted to sail up the lakes, they should remember that the extent and force of the westerly winds which are to follow may be estimated by the

extent of fall in the barometer; but not at all by the illusive moderation of the first or easterly winds of the cyclone. The fall of the barometer on the lakes, owing to the increased elevation, is less than on the ocean from a like storm; yet it is sometimes one inch, or even more; and whenever it exceeds the ordinary mean of moderate storms, every precaution should be taken.

Mr. Redfield then alluded, briefly, to the aneroid barometer, as a convenient and portable substitute for the common barometer, but occasionally requiring a verification.

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2. Notice of the Hail-Storm which passed over New York City, on the 1st of July, 1853. By Professor Elias Loomis, of New York University.

On the 1st of July, 1853, a very remarkable hail-storm passed over the city of New York. The day had been uncommonly hot and sultry, the thermometer having risen to 90 degrees, and the air was believed to contain an unusual amount of vapor. A little before five o'clock in the afternoon, a heavy black cloud was observed to rise in the northwest, the wind at the time blowing moderately from the northeast, and subsequently from the east. As the cloud advanced and covered the northwestern sky, while it was still clear in the southeast, numerous streaks of zigzag lightning appeared to issue from the front margin of the cloud, and descend toward the earth. I noticed the approach of the storm from my lodgings in Eighth Street, within a quarter of a mile of the University. About five o'clock the wind came strong from the northwest, and the rain poured down in torrents. Presently I heard a loud thump upon the roof of the opposite house; soon another thump; and presently a third and fourth. I was not long in discovering that the noise was produced by the fall of hailstones of a size such as I had never before witnessed. They were few in number, but their average size was little less than that of a hen's egg; and one or two, I am persuaded, were fully as large as my fist. They almost invariably broke on striking the pavement; so that I could not secure either of those large stones except in frag-

ments; and, moreover, the rain was falling in torrents. I, however, hastened to the yard in the rear of the house, hoping to find some upon the grass which had not been broken in the fall. After the rain had nearly subsided, we found several handfuls of hailstones of good size, though altogether inferior to those which I saw in the street. They generally consisted of very transparent and solid ice, with many air-bubbles; but they were not spongy in the centre, as they are sometimes found. Hailstones sometimes occur which appear to be little more than pretty compact snow-balls. In the present instance the hail was *not* of this kind. The large stones, however, generally consisted of an irregular assemblage of angular pieces of ice, which individually did not much exceed the size of hazel-nuts; but they were cemented firmly together. Indeed, there was no appearance of seams or joints between these individual portions; but the ice was equally strong throughout every part of the mass. The structure, therefore, did not indicate that several small hailstones were separately formed and were subsequently cemented together; but rather, that all were formed simultaneously about a common nucleus. Several persons independently, and without concert, suggested that the conglomerated mass resembled rock-candy; and the comparison appeared to me to be a very just one. There was a decided appearance of a tendency to crystallization. This tendency was in many cases toward a pyramidal form; in others, they bore a resemblance to hexagonal prisms; and in some, it appeared to me the tendency was towards a cubical form, though, as the angles were all much rounded by the melting of the ice, I did not find any complete cubes.

Several of the stones which we picked up in the yard measured two inches long, and one measured over two and a half inches. These had been lying several minutes in a warm, drenching rain; and it is my full conviction, that two or three of those which I saw in the street were three and a half inches long, by two and a half inches wide, and they did not appear to deviate much from the spheroidal figure. A friend of mine, who is by profession a painter, and who saw and handled the hailstones, at my request made a sketch of some of the most remarkable of those which we picked up. These are shown in the accompanying figures, which are drawn of the natural size. It is to be understood, however, that these were unquestionably smaller than many which we saw fall.

Figures 1 to 4. — Hailstones.

Figures 5 to 8. — Hailstones.

The rain, accompanied by thunder and lightning, continued for six or eight minutes, when its violence somewhat abated; it returned again with renewed energy, but soon afterwards entirely ceased. Another, but more moderate shower, followed half an hour later, yet without either hail or lightning. Throughout the entire storm, the wind had blown with considerable force, but not with destructive violence, in that part of the city which is southwest of the University; and in the lower part of the city there was comparatively little wind.

In the upper part of the city, however, in the neighborhood of the Crystal Palace, the wind blew with destructive violence. A high brick wall was blown flat to the ground; a block of four wooden buildings (not entirely completed) was prostrated; and a small wing of the Crystal Palace was blown down. The fall of hail was heavy, and considerable glass in the Crystal Palace and the buildings in the vicinity was broken.

During the first part of the storm, the lightning was unusually severe. Several buildings and trees in New York and Williamsburg were struck by the electric fluid, and one or two barns were burned to the ground.

I have succeeded in tracing this storm for a distance of full twenty-five miles, and for about two thirds of this distance have followed the track personally on foot. The portion of the track which I have myself surveyed commences about a mile and a half southwest of Paterson, N. J., from which point it proceeds in a southeast direction, passing over the village of Acquackanonck, together with the cities of New York and Williamsburg; and from this point the storm can be traced with diminished energy to Jamaica Bay. Near Paterson, the wind is believed to have been more violent than in any other part of the above-mentioned track. Where it swept through the forests, many large trees, of one to two feet in diameter, were overturned, while others were snapped off and twisted like reeds. This remark applies to a distance of about three or four miles from the commencement near Paterson. In the neighborhood of Acquackanonck, a few trees were overturned, but not a large number. East of Acquackanonck, the track soon crossed the Hackensack meadows, where the ground is low and flat, and there were no trees to be overturned. I have obtained no information of the effect of the wind upon

the high ridge on the west bank of the Hudson River; but the entire length of the track across New York was marked by violence, as above stated. This region was particularly exposed, since it was the highest ground encountered by the storm in its passage across the island. Having crossed the East River, the storm passed centrally over Williamsburg, where it caused more damage than in any other part of its course. The steeples of two churches (the First Presbyterian and the Dutch) were blown down; the roof of a third church was partially blown off; the roofs of a large number of dwelling-houses were carried away; the tin from numerous roofs was rolled up in long solid coils, and in some cases carried off, while in others it clung to the roofs, and was left after the storm in long, massive windrows.

The breadth of the track near Paterson is thought not to have exceeded half a mile, — perhaps was somewhat less than this; and the destructive violence did not extend beyond these limits, until the storm reached Williamsburg: but here the wind was almost equally violent over a space a mile and a half in breadth, while houses were unroofed over a track two miles in breadth. Beyond Williamsburg, the wind was less destructive; the track became broader and less distinctly defined; and the general course deviated more to the east. The storm was reported as severe at Jamaica and South Jamaica. From the commencement, near Paterson, to Williamsburg, the track did not deviate sensibly from a straight line; and its course was from N. 40° W. to S. 40° E.

Throughout the entire track here mentioned, hail fell of unusual size. Near Paterson the stones were smallest in size, but most abundant in quantity. The destruction caused to the fruit and the crops was such as not unfrequently occurs in France, but has seldom been witnessed in this country. When I visited the spot, a few days after the storm, the trees looked as if they had been pelted by myriads of heavy stones. The leaves were strewed thick upon the ground; and most of those which still clung to the branches were riddled through and through, and dried upon the stems. The rails of the fences bore marks of large gashes, where the brown, weather-worn surface had been nicked off, and a fresh surface exposed, as if by a volley of stones from a troop of mischievous boys. Upon the north side of the houses along the track, scarce a pane of glass was left entire, and the clapboards were covered thick with gashes an inch in diameter, where the

paint was chipped off. Fields of wheat and rye, which had not been harvested, were beaten down as flat as if a heavy iron roller (such as is sometimes employed for smoothing gravelled walks) had been dragged over them; and fields of corn were totally destroyed. I was assured that, after the storm, on some fields, the ice lay in a solid compact mass two inches thick. Large quantities of ice still remained unmelted on the ground the next morning, and a tenant on one of the farms collected a considerable quantity and carried it into Paterson, two miles distant, to show to his landlord; and I was informed that in a hollow, against the side of a house, the ice accumulated to such a depth, that on the evening of July 2d, the day after the storm, a bushel-basket full of ice was shovelled up; the stones varied in size from that of a pigeon's egg to a hen's egg. The track of the ice did not deviate much from the track of greatest violence of the wind, and followed the same general direction, but covered a somewhat greater breadth. From Paterson to the Hackensack meadows, beyond Acquackanonck, the damage caused by the hail was very great, amounting to nearly an entire destruction of the crops of wheat, rye, oats, and corn, as also the cherries, peaches, apples, etc., within the limits of the track.

In the city of New York, the damage done by the hail was not very great; for the stones were not numerous, although of prodigious size. The ship-yard of Mr. Thomas Collyer, at the Dry Dock, was covered with singularly shaped pieces of ice, — one of which was measured and found to be $6\frac{1}{4}$ inches in circumference, another seven inches, and a third measured three inches long and two inches thick.

In Williamsburg the hail appears to have destroyed more glass than in New York. In many houses nearly half the glass was broken in windows which were unprotected on the north side. Over four hundred panes of glass were broken from the north side of a single school-house.

On the same day with the preceding storm, large hail is said to have fallen at several places in Pennsylvania. About three o'clock in the afternoon, a terrific hail-storm passed over Northumberland, doing great damage. Hailstones are said to have been picked up, measuring $7\frac{3}{4}$ inches in circumference, and several thousand panes of glass were broken in that town.

At $5\frac{1}{2}$ o'clock, P. M., a severe hail-storm passed about 20 miles

north of Philadelphia. At Upper Dublin, the storm was very destructive. Several barns were unroofed, many fruit and forest trees were blown down, and many fields of wheat and oats so badly damaged as scarcely to pay for harvesting. One hailstone was measured, and its greatest circumference found to be $6\frac{1}{2}$ inches, and its smallest 5 inches, — and this was half an hour after the storm had abated. At Norristown and Doylestown the crops were much injured by the hail, and at Burlington, N. J. the wind was exceedingly violent.

It is not probable that either of these storms was the same as that which passed over New York. The hail-storm near Philadelphia was about simultaneous with that at New York. The storm at Northumberland may have been identical with that at Upper Dublin, the distance of the places being one hundred miles, the interval of time two and a half hours, and the direction nearly parallel with the track of the New York storm.

It would appear that a violent wave of great extent set in from the northwest, which rolled over both New York and Philadelphia, and within this wave were formed, about simultaneously, several distinct veins of hail.

Was the storm which passed over New York a whirlwind?

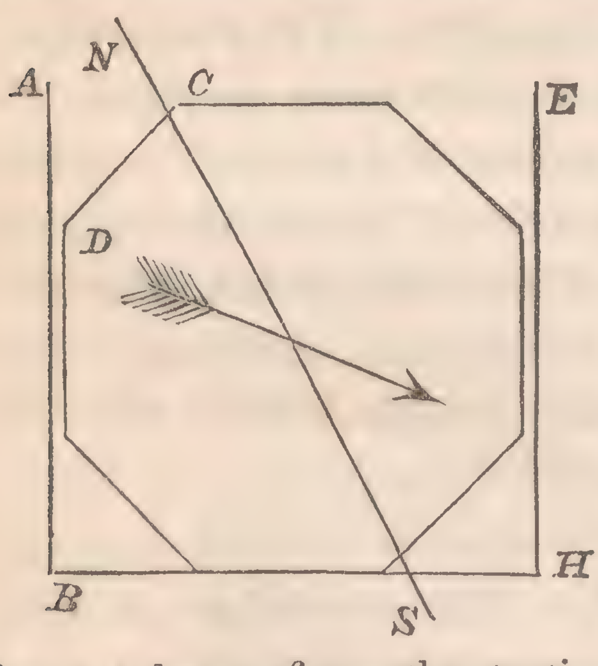
I have surveyed every part of the track of the storm where I have heard of any violent effects, especially with reference to the decision of this question. Throughout Williamsburg, I could find no unequivocal evidence of rotation. The steeples which were prostrated fell in a direction coinciding very nearly with that of the storm's progress, that is, towards the southeast. In the case of one of the churches whose steeple was blown down (the First Presbyterian Church) I noticed a phenomenon which I considered worth recording. The track crossed the ridge of the church at an angle of about 45°. On the leeward side, the tin roof was started from the boards, but not broken, and puffed up, forming a wrinkle about twenty feet long, two or three feet wide, and ten inches high. This appears to me to indicate the operation of a force beneath, pushing up the tin; but which, not being able to tear the tin open, bulged it up and left it in a ridge.

This phenomenon appears to be analogous to what often occurs in tornadoes, and I ascribe it to a rarefaction of the air on the leeward roof. A current of air, forcibly impelled over an obstacle like the roof of a building, by friction drags along with it the air lying upon

the leeward side of the roof, producing a partial rarefaction, and the air beneath, by its expansion, tends to lift the roof. Thus the leeward roof is often carried away, while the windward roof remains. In the present case, this upward pressure lifted the tin about ten inches, stretching, but not tearing it. This force appeared to be insufficient to tear the tin from its fastenings; perhaps because, from the carrying away of the steeple, and the ripping up of the adjacent edge of the tin, the air beneath found a ready escape.

In the neighborhood of the Crystal Palace occurred a phenomenon which appeared to indicate the existence of currents blowing nearly in opposite directions. The wooden buildings which have already been mentioned were blown toward the southeast, but the brick wall, the line of which ran from N. 28° E. to S. 28° W., fell toward the west; that is, in a direction nearly contrary to that of the storm's progress.

The following appears to me to be the explanation of this phenomenon. The Latting Observatory is an octagonal tower, 300 feet high and 75 in diameter at base, sloping uniformly to the top. In the following figure, the octagon represents the base of the tower, and the line *N S* represents a meridian. On the west side of the tower was erected a brick wall *A B*, twenty-five feet high, and only three feet from the side of the tower. At the south end, it was connected with another wall *B H*, but at the north end it was entirely free, and had no support except an iron clamp projecting from the side of the tower. The direction of the storm's progress is indicated by the arrow. It might have been anticipated that the wall *A B* would have been thrown towards the east upon the tower; whereas, in fact, it was thrown outward toward the west.



But we know from the testimony of spectators, that this wall fell at the first onset of the blast, when, as we shall presently see, the wind blew nearly from the north, or perhaps a little east of north. Now a violent current from the north, driven into the triangular space *A D C*, would necessarily become condensed between the wall and the tower, exerting a force to push the wall outward; and the wall had little

strength to resist the pressure, being not only weak, from its great height, but also because without support at its north end. The bricks, also, had been recently laid, and the mortar was not yet dry. On the east side of the tower was a similar wall *E H*, but only fourteen feet high, which was not prostrated. Its security is probably to be ascribed to its inferior height.

The following facts at first seemed to me a little puzzling. Near Paterson, a large oak limb, a foot thick, was twisted off at the height of fifty feet, and prostrated in a direction pointing S. 20° E., the top of the limb lying towards the base of the tree. Within a short distance I found another large oak limb, torn off at a great height and thrown towards S. 40° E., with its top also turned towards the base of the tree. Not far off, I found a third limb in a similar position. Broken limbs were generally found to have been carried eastward, with the top pointing to the southeast, and the base towards the northwest. The three cases I have here specified were exceptions to the general rule, and it appears to me that they are to be explained by supposing that the branches turned a somerset in falling.

A like case occurred with the steeple of the First Presbyterian Church in Williamsburg. The spire fell across the street, — the top struck a brick house on the opposite side of the street, and broke off, while the upper portion of the spire remained imbedded in the roof of the house, which was crushed in by the blow. The remainder of the spire, which was now the frustum of a pyramid, fell down into the street; but it is probable that the smaller end of the frustum struck the pavement first, for the steeple turned a somerset along the street towards the east, and lay after the storm with the smaller end of the frustum turned towards the church.

A similar supposition will satisfactorily account for the observed position of the three limbs above mentioned.

In the woods, between Acquackanonck and Paterson, I measured with a pocket-compass the direction of a large number of prostrate trees. The following list shows the entire range of the bearings which I measured; not arranged in the order in which they were measured, but classified according to the points of the compass. They were

S. 70° W.; S. 20° W.; S. 15° W.; S. 10° W.; South; S. 10° E.; S. 20° E.; S. 25° E.; S. 30° E.; S. 35° E.; S. 40° E.; S. 45° E.; S. 50° E.; S. 60° E.; S. 70° E.; East.

These bearings were measured at various points upon a portion of the track about two miles in length; and it will be noticed that there is not a single instance of a tree which was prostrated towards any point between east, north, and west. The bearings extend from east, through south, to S. 70° W., a range of 160 degrees; but I found only one instance of a bearing approaching nearly so close to the west point. With but one exception, the bearings were all confined between east and S. 20° W. The wind did not then blow from every point of the compass indifferently, at least not with sufficient force to prostrate trees, but it blew only from the northward, including northeast and northwest. Neither was the wind a simple rectilinear current. What law, then, did the wind observe? Was its motion merely centripetal? Did it revolve in a whirl? Or did it follow some other law?

In order to decide these questions, I attempted to apply the method which I had successfully practised in the Mayfield tornado of February, 1842. This method consisted in selecting groups of prostrate trees, which lay upon each other, and measuring successively the bearings of the bottom tree, of the next in order, and so on to the top, and regarding these bearings as indicating the successive directions of the wind at the point of observation, as the storm passed by. At Mayfield I had no difficulty in finding groups of trees piled upon each other, frequently four or five in a group; but at Paterson I found in no case more than two trees crossing at a considerable angle, and only five instances of this description. The following are the observations in the cases referred to, the bearing first mentioned in each case being that of the bottom tree.

$$1.\left\{\begin{array}{l}\text{S. } 50^{\circ}\text{ E.}\\ \text{S. } 20\text{ E.}\end{array}\right. \quad 2.\left\{\begin{array}{l}\text{S. } 40^{\circ}\text{ E.}\\ \text{S. } 25\text{ E.}\end{array}\right. \quad 3.\left\{\begin{array}{l}\text{S. } 20^{\circ}\text{ E.}\\ \text{S. } 40\text{ E.}\end{array}\right. \quad 4.\left\{\begin{array}{l}\text{S. } 40^{\circ}\text{ E.}\\ \text{S. } 10\text{ E.}\end{array}\right. \quad 5.\left\{\begin{array}{l}\text{S. } 70^{\circ}\text{. W.}\\ \text{S. } 60\text{. E.}\end{array}\right.$$

The first four cases present no angle greater than 30°; the fifth case presents an angle of 130°; that is, the two trees were turned in nearly in opposite directions.

From a comparison of all the facts, I conclude that the wind blew first from the northeast, and that this current was succeeded by a north, and presently a northwest wind. The following are my reasons for this conclusion: —

1. The fifth case of interfering trees, just mentioned, taken in connection with all the bearings observed, points to this conclusion. We

find that one large tree was prostrated with its top turned towards a point S. 70° W. Upon it lay another large tree, with its top turned S. 60° E. We may safely infer, that these directions corresponded nearly with the direction of the wind when the trees were prostrated, and that the wind came first from a point N. 70° E., and was succeeded by a current from N. 60° W. In each of the other cases of interfering trees, the angle of crossing was so small, as to convey no very distinct information on this question. In three cases out of the four, the first blast appears to have been a little more westerly than the final one ; but all the trees were prostrated by a northwest wind.

2. A very intelligent farmer, whose house was close upon the northeast margin of the track, about four miles from Paterson, gave the following testimony. He first took refuge from the hail under a shed, on the southwest side of the barn, the wind then blowing from the northeast. After a short time, the hail began to beat upon him, the wind having veered to the northwest, and he was obliged to seek a shelter on the southeast side of his barn, in order to escape the hail.

3. It is known from the testimony of several individuals, that the wind at New York was easterly on the first appoach of the storm.

Upon comparing these facts, it appears to me that the direction of the wind at the time of its most destructive violence may be tolerably well represented by the following diagram, showing a current from

the northeast, on the front of the storm; and from the northwest in the rear, the whole having a progressive motion towards the southeast, which would give to each place in succession (unless near the southwest margin of the track) first a northeast wind, and afterwards a northwest wind.

I do not then find in this case the evidence of a complete rotation, which I have found in some other tornadoes; but it is possible that, at a little elevation above the earth's surface, the rotary motion may have been more decided.

What was the cause of the hail?

The hail was caused by a violent upward movement of the air, carrying along with it an unusual amount of vapor, which was suddenly condensed, and at so low a temperature that it was frozen in large semi-crystalline masses.

That there was a violent upward movement of the air, I infer from the following considerations.

1. Rev. J. W. McLane, of Williamsburg, was in the street near his house, and noticed the coming up of the storm. He says the cloud was very dense and black, moved rapidly forward, and under the main sheet the clouds boiled up in a violent and angry manner, which led him to anticipate a severe blast. Other observers have testified to substantially the same facts.

2. It appears impossible that two currents, in close juxtaposition, should blow from nearly opposite quarters, with sufficient violence to prostrate large trees, unless there is opportunity for the air to escape by an upward movement. This conclusion is also in perfect harmony with what we have frequent occasion to observe in small sand-whirls and water-spouts.

How was the cold which formed the hail produced?

According to the observations of Pouillet, in France, the temperature of hailstones, when they fall, is sometimes as low as 25° Fahr. They must have been formed at a temperature considerably below that of melting ice, — a temperature, probably, as low as 20° Fahr. How can such a temperature be produced in the hottest weather in July? The temperature of the air diminishes as we ascend from the earth, and at the height of 8,800 feet above New York is estimated at 32° in summer. At the height of twelve thousand feet, the temperature is reduced to 20°. Were the hailstones formed in the

present case at an elevation of twelve thousand feet? It does not appear to me that we are at liberty to make such an assumption. In the summer of 1835, several hail-storms passed over the southern part of France, where there were insulated peaks of mountains, which afforded precise means of measuring the elevation of the hail. In the storm of July 28th, 1835, no hail fell on the summit of the Puy du Dome, an elevation of 4,800 feet above the sea; but a few stones fell at the height of 3,700 feet, while at the foot of the mountain the ground was covered to the depth of three inches, and some of the stones weighed eight ounces. On the 2d of August of the same year, a hail-cloud enveloped the summit of the mountain, rising therefore to the height of at least 5,000 feet.

It does not, therefore, appear to me that we are at liberty to assume that the hail of July 1st was formed at an elevation much exceeding 5,000 feet, and here the summer temperature may be estimated at 46°. This cold is, of course, insufficient to form ice.

It is believed that, during the passage of a hail-storm, the temperature of the upper air is considerably below the mean. The simple presence of clouds in the lower atmosphere would tend to produce such an effect. The atmosphere derives its heat from the earth, and is but little affected by the direct passage of the solar rays. The heat which the earth imbibes from the sun is continually thrown off by radiation; but when the surface of the earth is covered by a cloud, this radiant heat is intercepted, and the temperature of the lower air is thereby elevated. On a still night, the presence of clouds sometimes causes the thermometer to stand ten or fifteen degrees higher than it would otherwise. But if, by the interposition of a cloud, the lower atmosphere becomes unusually hot, the atmosphere above the cloud must receive less than its usual supply of heat, that is, it must become unusually cold.

Moreover, in the storm of July 1st, the hail was formed in a current blowing violently from the northwest, which came, therefore, from a higher latitude, and, of course, brought with it a diminished temperature. I have no data sufficiently precise for estimating the effect to be ascribed to this cause, but I think we may conclude that, at the time of the storm in question, at an elevation of 5,000 feet above New York, the temperature could not have varied much from 32°. We have not, however, yet reached the temperature necessary to the production of hail.

Another source of cold is to be found in the evaporation from the surface of the hailstones. It is well known that, if we tie a piece of thin muslin upon the bulb of a thermometer, and then, after dipping the bulb in water, swing it rapidly through the air, the thermometer will sink below the temperature of the air, several degrees, sometimes ten or fifteen; an effect due to evaporation. During a hail-storm, the hot air from the earth's surface is carried, by the upward movement, to a considerable elevation; by expansion it is cooled, and a portion of its vapor is condensed. The drops thus formed, at a temperature not far from 32°, are still further cooled by evaporation from their surface (the evaporation being promoted by their rapid motion); the remainder of the drop is congealed, and as new vapor is precipitated, it is congealed upon the former, thus forming concentric layers round the nucleus. Since water, like nearly every other substance, in passing to the solid state, inclines to crystallization, the ball, as it increases, does not generally retain the spherical form, but shoots out irregular prisms.

How does a hailstone remain suspended in the air long enough to acquire a weight of half a pound?

This difficulty is not, to my mind, a very formidable one. I conceive that hailstones are formed with great rapidity. The vapor is condensed with great suddenness, and almost instantly frozen. I think very large hailstones may be formed in five minutes. In a vacuum, a stone would fall from the height of 5,000 feet in less than 20 seconds; but drops of water and hailstones fall with only a moderate velocity. From my own observations of the hailstones of July 1st, I estimated the velocity of their fall at about 40 feet per second. At the uniform rate of 40 feet per second, a stone would be more than two minutes in falling 5,000 feet.

In order to obtain some reliable data for estimating the velocity of hailstones, I have computed the greatest velocity of a number of small bodies, differing in size and specific gravity. Dr. Hutton determined, by numerous experiments, the resistance of the air to bodies moving with different velocities; and in the third volume of his Tracts, p. 218, has given a table of the air's resistance to a sphere 2 inches in diameter. His experiments also indicated that the resistance of spheres increases in a ratio somewhat greater than the squares of the diameters. This excess for spheres of from 2 to 4 inches in diameter

was about one thirtieth part of the resistance. The second column of the following table is taken from Hutton's Tracts, the resistance for velocities from 50 to 100 being supplied by interpolation. The resistance for a sphere one inch in diameter is found by taking one fourth of the numbers in the second column, and diminishing the result one thirtieth part. Each succeeding column is derived from the preceding in a similar manner.

Resistance of the Air to Spheres of different Diameters.

Velocity per second.	Sphere two inches in Diameter.	Sphere one inch in Diameter.	Sphere one half inch in Diameter.	Sphere one fourth inch in Diameter.	Sphere one eighth inch in Diameter.
Feet.	Ounces.	Ounces.	Ounces.	Ounces.	Ounces.
5	0.006	0.001	0.000	0.0001	0.0000
10	0.026	0.006	0.001	0.0004	0.0001
15	0.058	0.014	0.003	0.0008	0.0002
20	0.103	0.025	0.006	0.0015	0.0004
25	0.163	0.039	0.010	0.0023	0.0006
30	0.237	0.057	0.014	0.0033	0.0008
35	0.325	0.078	0.019	0.0046	0.0011
40	0.427	0.103	0.025	0.0060	0.0015
45	0.544	0.131	0.032	0.0077	0.0019
50	0.676	0.163	0.039	0.0095	0.0023
55	0.821	0.198	0.048	0.0116	0.0028
60	0.981	0.237	0.057	0.0138	0.0033
65	1.155	0.279	0.067	0.0163	0.0039
70	1.343	0.325	0.078	0.0190	0.0046
75	1.546	0.374	0.090	0.0218	0.0053
80	1.764	0.426	0.103	0.0249	0.0060
85	1.996	0.482	0.116	0.0282	0.0068
90	2.243	0.542	0.131	0.0317	0.0076
95	2.505	0.605	0.146	0.0354	0.0085
100	2.780	0.672	0.162	0.0392	0.0095
200	11.340	2.764	0.668	0.1615	0.0390
300	25.800	6.235	1.507	0.3641	0.0880

In a vacuum, a body falling under the influence of gravity is continually accelerated, but when a heavy body falls through the atmosphere, the resistance increases with the velocity, until the resistance becomes equal to the weight of the body. When this takes place, there can be no further increase of velocity, and the body will afterward descend with a uniform motion. In order, therefore, to determine the greatest velocity which a heavy body can acquire by falling through the atmosphere, it is only necessary to compute the weight of a sphere of given diameter, and then to search in the preceding table for the velocity due to an equal resistance upon a body of the proposed diameter. The following table exhibits the results for spheres of lead (assuming its specific gravity 11.35), of iron (spe-

cific gravity 7.78), of water, of ice (specific gravity 0.93), of cork (specific gravity 0.25); the diameters varying from two inches to one eighth of an inch.

Diam.	Weight of a Sphere of					Final Velocity of Sphere of				
	Lead.	Iron.	Water.	Ice.	Cork.	Lead.	Iron.	Wat.	Ice.	Cork.
Inches.	Ounces.	Ounces.	Ounces.	Ounces.	Ounces.	Feet.	Feet.	Feet.	Feet.	Feet.
2	27.5132	18.8593	2.4241	2.2544	0.6060	310	257	94	90	47
1	3.4392	2.3574	0.3030	0.2818	0.0757	223	185	68	65	34
$\frac{1}{2}$	0.4299	0.2947	0.0379	0.0352	0.0095	161	134	49	47	25
$\frac{1}{4}$	0.0537	0.0368	0.0047	0.0044	0.0012	117	97	36	35	18
$\frac{1}{8}$	0.0067	0.0046	0.0006	0.0006	0.0001	84	70	25	24	12

Thus it appears that a hailstone, in the form of a sphere, two inches in diameter, falling through a tranquil atmosphere, cannot possibly acquire a velocity exceeding 90 feet per second, and spheres of a smaller size would acquire a still less velocity. Also, a hailstone of irregular shape would experience more resistance than a sphere, and would acquire a smaller velocity in falling. An upward current of air, moving with a velocity of 90 feet per second, or 61 miles per hour, would sustain a sphere of ice two inches in diameter; also, an upward current of 30 miles per hour would sustain hailstones of half an inch in diameter, and would greatly reduce the velocity of stones of larger size. The strong upward movement which is known to exist in the neighborhood where hail is formed is, therefore, quite sufficient to sustain hailstones of the largest kind as long as they can be kept within the influence of this vortex. After they have entirely escaped from the influence of the vortex, small stones would fall to the earth, from an elevation of 5,000 feet, in about two minutes, and very large stones in one minute. I see no difficulty, therefore, in supposing the great mass of hail to remain in the air five minutes before reaching the earth, and that, in peculiar cases, stones may remain supported for ten minutes, and even longer. This period appears to me to be sufficient to account for the hail which fell at New York.

Why did the hail in the present case attain to such unusual size?

Because of the following circumstances, which are unusually favorable to its formation. The temperature of the air, before the storm, was 90°, and it is my opinion that the dew-point could not have been less than 80°; in other words, the air contained about as much vapor as it is ever known to contain in this latitude. This vapor was sud-

denly lifted to a region of great cold, and rapidly condensed and frozen. The strong upward movement helped to sustain the crystals as they increased in size, until the upward force was no longer equal to gravity, or until they escaped from the influence of the vortex. Most of the stones would fall in five minutes, and be of moderate size; others might be sustained ten or fifteen minutes, and attain enormous dimensions.

How did the hail in this storm compare with the most remarkable cases on record?

There are well-authenticated cases of hailstones having fallen in England and France weighing half a pound, and even more than this; but the accounts of hailstones weighing so much as one pound do not appear to me entirely satisfactory. A mass of ice of the specific gravity 0.93, weighing eight ounces, must contain nearly fifteen cubic inches; or would make a cube whose edge is nearly 2.5 inches. I have selected a piece of ice which was estimated to be about the size of the largest stone which I saw fall on the 1st of July, and found it to weight eight ounces. But these large stones of July 1st appeared to me unusually white, and may therefore be conjectured to have had a spongy nucleus, which would have reduced the weight to perhaps six ounces.

The hail, therefore, in the present storm was somewhat smaller than has been observed to fall in other places.

Since the preceding was written, I have received notice of several remarkable hail-storms in different parts of the United States, two of which were so extraordinary that I have added an account of them to this paper.

Hail-Storm experienced at Warren, N. H., August 13, 1851.

My first information respecting this storm was derived from a letter from Dr. Peter L. Hoyt, dated Wentworth, Grafton Co., N. H., Aug. 3, 1853. The following is an extract from Dr. Hoyt's letter: —

"Perhaps a brief notice of a hail-storm which occurred in this vicinity on the 13th of August, 1851, may be of interest to you. This shower, about one o'clock, P. M., passed from the west towards the east over an extent of four or five miles around the base of Moose-

hillock Mountain, in the towns of Benton and Warren. The largest and most hail fell in the northeast part of the latter town, in a basin between the mountains, near the source of a stream called Baker's River. I stood at the railroad depot in Wentworth, at the time of this shower, distant in an air-line six or seven miles. It was the most remarkable appearing cloud, I think, I ever saw, — so black and dense, encircling and covering the mountain, and shutting down to the earth.

"The hail was of prodigious size and in great quantities. The largest of the stones were of an irregular shape, rough and angular, suggesting the idea to some that they were made up of a number adhering together. They were, however, very solid, and not easily broken.

"One was weighed upon the spot at the time of the shower, and weighed twenty ounces; and the person who informed me of this was of the opinion that he saw one fall and break in pieces which was still larger. They looked, he said, like vast pieces of ice that had been broken above, and were falling to the earth. A quantity was gathered in a basket and brought to Warren village, a distance of three or four miles, and there exhibited at least an hour after the shower, and in a hot, sultry afternoon. One of them there weighed fourteen ounces, and measured ten inches in circumference. Twelve of the largest taken out of the basket weighed, on the counter scales in the store, seven pounds.

"About four o'clock, P. M., three hours after their fall, a box of them was brought to Wentworth village, where I reside, a distance of about eight miles. One of them was shown to me. Its diameter, according to my best judgment, was from 2 to 2½ inches. It had the appearance of being originally somewhat angular, with the angles melted off. It was perfectly solid and clear.

"So large was the quantity of hail in many places in Warren, that a cart-load might have been gathered without moving from the place. Luckily the track of the storm was not through the most cultivated part of the towns, but along the borders and skirts of the forests, where the population was scattering. Crops of hay and grain were ground to the earth, poultry were killed, cattle's backs were bruised, and the roofs of many buildings were badly broken. But little glass was broken, from the fact that the direction of the hail was nearly perpendicular to the earth."

Immediately upon receiving this letter, I wrote to Dr. Hoyt, stating

that the facts which he had communicated respecting the size of the hail were so remarkable, that they ought to be substantiated by such evidence as would be deemed conclusive in a court of justice; that it was therefore important that he should obtain written statements from the identical persons who weighed the stones; and that it would not be derogatory to the dignity of science for these persons to make affidavit to the truth of their statements before a justice of the peace. I also suggested several additional points upon which it was desirable that information should be obtained. In reply I received another letter from Dr. Hoyt, accompanied by documents such as I had suggested. The following is extracted from his reply: —

"As yet I have been unable to substantiate the weight of a hailstone at twenty ounces; yet throughout the town of Warren the impression prevails that one was so weighed. The enclosed affidavit of Mr. Libby, and the statement of Mr. Flanders, fix the extreme weight of two stones weighed by them at $17\frac{1}{4}$ and 18 ounces, with the firm belief of Mr. Libby that, had he weighed them at the time of falling, their weight would have been increased some two or three ounces.

"I have the names of some two or three other individuals who weighed several stones of a pound weight; others were weighed, weighing three fourths of a pound. One several hours after the storm weighed fourteen ounces. A tin pail full containing fifteen weighed ten pounds; four collectively weighed three pounds, etc. Incredible as the above may appear to some, they are facts which can be proved by a multitude of evidence.

"This storm was remarkable for the amount of ice which fell, as well as the size of the stones. Mr. Flanders thinks that in Benton the average depth of the hail was about four inches, and, from inquiry along the track of the storm, I should judge that he is not far from right in his estimate. The extreme width of the hail was about two miles, and the length over a cultivated district perhaps about five or six miles. How far east it extended I have no means of knowing, as it entered a forest of many miles in width. The largest hailstones fell near the edge or skirts of its track; the thickest and greatest amount or depth of hail fell in the centre. Although the sun came out 'boiling hot,' as one man expressed it, after the shower, still the hail remained on the ground in many places until the next day. An

owner of a saw-mill on the stream of water which has its source in the forests over which the shower passed, told me that the water kept swollen for two or three days, when from common showers of rain it would have fallen in twenty-four hours. This he attributed to the gradual melting of so large a quantity of ice in the woods. I think there was but little if any difference in the distribution of the large stones along the track, as the two whose weights are given by Mr. Flanders and Mr. Libby were picked up about five miles apart, and near the extremes of its track before it entered the forest. On the borders or skirts of the cloud the large stones fell scattering, and as it approached the centre it was as if the whole contents of the cloud were let down in ice. During the time of the hail, which lasted some twenty or thirty minutes, there was but little rain; after the hail, it rained briskly for ten or fifteen minutes.

"*Shape of the Hail.* — In Benton, at the commencement of the hail, the masses were angular, having a resemblance to broken ice; while farther along the track they assumed a smoother and more uniform surface, being oval or oblong. In many instances the surface is described as being notched or scalloped; and in some few, as being covered with icy spikes, like icicles, somewhat resembling a bur. It is the opinion of those who examined these stones the most minutely, that they were not formed by the union of several masses, but were distinct and individual in formation. They were compact and very solid; so hard, that they might be thrown with great force against a house and not be broken.

"*Velocity.* — All agree that the hail fell with great velocity and force. Mr. E. W. Cleasby, a very correct and veracious man, whose statement is appended to this letter, says that hailstones very solid, and weighing in the vicinity of ten or twelve ounces, averaging one on about every two feet, fell in a piece of unmowed grass. In their passage through the grass they entangled it so as to carry it imbedded into the sward ground to the depth of some two or three inches, and, after the melting of the hail, left the turf full of holes like birds' nests. These holes remained through the season. As a test of the force necessary to effect this, he repeatedly, with a pitchfork-handle having a rounded head, tried to strike it into the ground to an equal depth, and was unable to do it.

"Many of the barns in this neighborhood have their roofs covered

with what are styled 'long shingles,'—that is, with spruce shingles without previous boarding. Whenever these large stones fell upon such roofs, they broke a hole completely through; and one man, having sought refuge in a barn under such circumstances, was obliged to hide under the scaffold. The marks and bruises upon the buildings caused by the hail are still to be seen. Says one person, 'They looked like little pumpkins falling.' The roar and rattle of the hail was distinctly heard at the village in Warren, a distance of four or five miles, and was likened to the noise of a heavy train of cars.

"*Wind.*—During the storm, there was but little wind. The hail fell nearly perpendicularly. The general bearing of the wind, as appears from my weather-table on that day, was west-southwest; and the direction of the shower was in correspondence with this.

"*Heat.*—No record of the heat was kept in the neighborhood of the storm. My thermometer at two o'clock, P. M. indicated 76°. I very well recollect that, after witnessing the passage of this cloud to the north, the sun broke out very hot and scorching. Such also is the testimony of people living there.

"You ask if it were possible that the larger stones could have been formed by the cementing or freezing together of several while lying on the ground? I should think it *impossible* that such could be the case. Furthermore, the general opinion among the inhabitants is that each stone was a unit in formation."

The following is a copy of the affidavit of Mr. Libby, already referred to:—

"*Warren, N. H., August* 24, 1853.

"I live in Warren, and witnessed the hail-storm on the 13th of August, 1851, between the hours of one and two o'clock, P. M. I weighed a number of the hailstones which fell at that time, but not until after the shower had ceased,—perhaps an hour and a half or two hours after. During this time it was very hot. The largest which I weighed was $17\frac{1}{4}$ ounces in weight. The others varied in the vicinity of a pound. I am fully in the belief, that, had they been weighed at the time of falling, their weight would have been some two or three ounces more. Previous to weighing them I washed the dirt from them in water. They were very irregular in shape, somewhat scalloped, with ice projections from their surface. I picked these stones up from soft ploughed ground where they were imbedded more

than half their size in the ground. During the time that the hail was falling there was but little rain, with little or no wind. After the hail there was a warm rain of some ten or fifteen minutes' duration.

"John Libby.

"Sworn to before me. Jesse Little, *Justice of the Peace.*"

The following is the statement of Mr. Flanders, already referred to: —

"*Wentworth, August* 30, 1853.

"I live in Benton, N. H., county of Grafton, and resided there at the time of the storm on the 13th of August, 1851. I weighed a number of the stones after the rain was over. The heaviest one weighed eighteen ounces; the others ranged in the vicinity of a pound. They were very irregular in shape, some nearly square, some scalloped, some angular, as if made up of several pieces. According to my best judgment, there was an average depth of four inches of hail which fell at that time.

"Granville E. Flanders."

The following is the statement of Mr. Cleasby: —

"*Warren, N. H., September* 3, 1853.

"This certifies that several of the hailstones which fell here on the 13th of August, 1851, were measured by members of my family. According to my best recollection, the circumference of the largest was fourteen inches one way and nine the other. Their form was generally oval.

"Ezra W. Cleasby."

The preceding evidence satisfies me that hailstones fell in New Hampshire on the 13th of August, 1851, weighing *more than one pound;* and I do not know of any satisfactory evidence that hail of equal size has ever been seen in any other part of the world.

Hail-Storm at Montrose, Iowa, on the Mississippi River, in Lat. 40° 30′, *June* 26, 1838.

The following notice of this storm is derived from a letter received from Mr. D. W. Kilbourne, who resided at Montrose in 1838, but now lives at Keokuk, twelve miles below Montrose.

"About five o'clock in the afternoon, a very heavy black cloud

rose in the northwest, the wind at the time blowing strong from that quarter. There was much thunder and lightning; at the same time, it was clear in the east and southeast.

"Very soon, however, the whole sky seemed to be covered by clouds; there was a heavy mist, and it was almost as dark as night. Rain immediately followed, and fell in torrents. Then hailstones began to fall. At first they were small, and excited no surprise in myself or family; but they continued to increase in quantity and size, to such an extent as to excite not only our wonder but our fears. The hail-storm continued nearly ten minutes, and during all the time small and large hail fell together. The wind was high.

"As soon as the storm abated so that it was safe to go out, my family were all engaged in picking up the stones. We then selected the largest, and measured their circumference. The largest one found measured *ten and one fourth inches.* There were a large number that measured from two to ten inches in circumference. I gathered up with my own hands in one spot on the grass, without moving, a half-bushel-measure full.

"Mrs. Kilbourne placed several of the largest ones upon the top of common-sized glass tumblers, and when melted they filled the tumblers so that some of them could not be moved without spilling the water.

"The hailstones were irregular in their formation, and presented very much the appearance of rock-candy. The ice was solid and transparent. We did not weigh them.

"The hail fell only about half a mile in width, and not more than two miles in length from west to east. No hail fell on the east side of the river. But few white families resided in the neighborhood at that time, or in the county; and I do not believe the hailstones were particularly noticed or measured, except at our house."

Professor Stoddard, of Oxford, remarked that hailstones of one pound weight had fallen in the city of Pittsburg, and with such force as to penetrate an iron roof. He also remarked, that, in several hail-storms, he had noticed the largest stones fall without the limits of the strongest violence of the wind; and hence he supposed they were formed whilst moving upward.

3. Does the Moon exert a Sensible Influence upon the Clouds? By Professor Elias Loomis, of New York University.

Sir John Herschel, in his *Outlines of Astronomy*, page 261, uses the following remarkable language: "The heat of the moon is much more readily absorbed in traversing transparent media than direct solar heat, and is extinguished in the upper regions of our atmosphere, never reaching the surface of the earth at all. Some probability is given to this by the *tendency to disappearance of clouds under the full moon*, a meteorological *fact* (for as such we think it fully entitled to rank) for which it is necessary to seek a cause, and for which no other rational explanation seems to offer." In a note on the same page he informs us that this fact rests upon "his own observations, made quite independently of any knowledge of such a tendency having been observed by others. Humboldt, however, in his personal narrative, speaks of it as well known to the pilots and seamen of Spanish America."

Having made a pretty extensive comparison of observations several years since, for the purpose of determining the influence of the moon's phases upon the fall of rain, I was led to distrust the preceding conclusion of Sir John Herschel, and have accordingly sought for observations by which its accuracy might be tested. For this purpose I turned to the Greenwich Meteorological Observations, where we find the amount of cloudiness of the sky recorded every two hours night and day for a period of seven years. I arranged all the observations in a tabular form, one column of which shows the average amount of cloudiness on the day of each full moon for the whole period; the second column shows the amount of cloudiness on the day after full moon; the succeeding columns show the degree of cloudiness on the second day after full, the third, fourth, etc. days of the third quarter; and other columns show the cloudiness for six days preceding the full. See the accompanying table. In like manner the observations are arranged for new moon, and also for the six preceding and following days. I then took the average of all the numbers in each column, and obtained the following results.

Date.	Full Moon.	Following Full Moon.						Last Quarter.	
		1 A.	2 A.	3 A.	4 A.	5 A.	6 A.		6 B.
1840	6.5	6.3	6.3	5.8	8.4	2.7		7.0	6.6
	9.3	0.4	8.0	10.0	9.7		6.1	5.2	[illegible]

AMOUNT OF CLOUDINESS AT GREENWICH, ARRANGED WITH REFERENCE TO THE PHASES OF THE MOON.

Date.	Full Moon.	Following Full Moon.						Last Quarter.	Preceding New Moon.						New Moon.	Following New Moon.						First Quarter.	Preceding Full Moon.					
		1 A.	2 A.	3 A.	4 A.	5 A.	6 A.		6 B.	5 B.	4 B.	3 B.	2 B.	1 B.		1 A.	2 A.	3 A.	4 A.	5 A.	6 A.		6 B.	5 B.	4 B.	3 B.	2 B.	1 B.
1840	6.5	6.3	6.3	5.8	8.4	2.7		7.0	6.6	9.6	7.3	2.8	6.8		8.8	5.5	4.8	0.1	0.0	3.1		9.2	5.3	1.3	4.8	10.0		9.1
	9.3	0.4	8.0	10.0	9.7		6.1	5.3	8.8	9.8	10.0		8.1	4.1	1.0	3.7	2.0	10.0		9.0	8.4	5.3	8.3	5.0		7.3	8.6	8.4
1841	2.7	6.7	3.6		8.7	5.3	6.7	10.0	10.0		10.0	8.9	5.9	3.8	5.3	5.8		2.8	9.7	7.9	5.2	5.5	10.0		8.3	9.3	6.6	9.9
	6.3	10.0		10.0	9.9	9.9	9.6	8.4		10.0	8.3	8.6	3.6	8.0	5.7		10.0	9.6	10 0	8.8	9.7	8.5	9.2	8.2	7.1	5.3	9.1	3.9
		5.9	1.5	5.3	0.0	0.0	2.5	1.7	0.1	6.1	6.0	2.9	5.3		6.8	5.8	6.0	1.6	5.3	4.8		9.2	6.8	7.4	9.7	5.1	4.7	
	8.6	7.2	9.3	8.3	5.0	9.7		8.5	4.1	4.3	4.3		6.5	7.8	7.8	9.3	8.7	6.0		8.8	2.2	4.3	1.5	3.8	0.0		10.0	9.0
	6.8	4.7	6.9	8.2		9.5	7.1	6.3	0.4	2.3		7.5	4.3	8.4	5.0	5.6	8.0		6.1	6.9	3.4	1.8	4.4	6.3		2.3	4.6	4.8
	3.3	3.5	8.1		7.8	10.0	9.0	5.3	8.5		2.8	8.5	2.9	4.9	6.0	7.8		5.3	5.5	6.2	6.1	6.9		10.0	7.5	6.9	9.5	10.0
	7.9		6.2	6.3	7.9	6.8	4.7	5.5	7.4	4.9	6.8	7.1	7.3	2.8		6.5	9.7	9.4	7.6	9.7	9.3		8.8	8.7	5.9	5.2	7.7	7.8
		9.0	9.5	9.0	8.0	9.0	7.5	6.1	5.5	9.7	4.2	7.8	7.6		6.1	9.2	6.2	0.7	0.8	4.3		8.0	9.3	7.6	6.2	1.8		1.1
	9.7	6.1	2.9	7.1	9.3		4.7	6.6	4.6	6.9	3.6	5.2		0.6	4.7	4.8	3.9	0.7	1.6		8.8	7.2	8.6	5.8		7.5	9.6	7.0
	7.6	9.3	4.3		9.2	6.5	6.7	3.3	8.3	8.5		5.3	7.7	7.6	8.7	6.4	7.4		4.9	5.2	1.4	2.9	8.3		5.5	8.6	10.0	10.0
	10.0	10.0		9.9	9.1	8.8	7.4	8.4	3.8		7.9	9.9	9.7	6.9	5.4	4.8		6.9	4.5	3.8	8.1	8.2	7.8	6.0	3 0	7.2	7.3	10.0
		9.6	8.3	8.3	7.7	7.8	7.4		8.2	7.6	8.3	5.8	8.1	2.3		6.2	6.8	8.5	2.8	1.4	0.0	7.3	6.0	6.5	7.6	7.8		
1842	7.0	8.1	10.0	8.5	8.1	8.8		5.4	9.4	8.7	6.0	9.7		10.0	9.9	8.8	10.0	9.8	5.0		2.7	8.5	10.0	10.0	9.2		2.6	7.1
	6.3	2.9	5.8	8.5		9.5	3.9	9.5	8.2	9.2	8.3		9.1	9.8	5.9	9.3	9.1	9.8		1.4	6.4	9.8	4.1	3.1		7.7	9.5	9.1
	8.9	6.8	3.5		7.6	6.7	7.3	9.0	3.1		7.0	8.5	7.4	5.3	9.7	3.3		10.0	10.0	9.7	9.2	6.6		6.3	8.0	2.8	8.9	
	3.4		7.9	8.0	6.1	9.4	8.6	7.1	8.0	3.5	0.2	5.9	2.3	2.7		5.6	8.8	10.0	9.6	6.6	3.9		9.9	5.3	0.1	5.3	5.0	4.5
		0.6	0.0	0.4	0.3	1.0	0.8	0.2	4.7	5.7	7.0	8.1	7.4		6.8	3.3	5.8	9.6	2.9	2.9		5.1	3.7	9.4	5.4	6.8	9.9	6.2
	4.0	7.8	5.0	6.8	9.3	5.5		4.3	4.0	0.2	1.6		3.3	3.3	2.2	0.2	0.0	0.1		2.5	7.9	5.1	7.0	8.1	9.1		6.8	7.9
	5.0	6.3	9.6	9.9		2.1	1.9	1.3	8.3	4.8		7.7	5.8	3.9	7.7	8.8	6.2		6.1	5.3	6.2	0.8	1.3	0.3		7.3	9.8	8.0
	6.8	8.3	5.0		2.8	1.5	8.9	6.2		5.3	2.5	6.7	3.9	5.9	9.2		2.5	0.9	5.1	4.4	1.0	5 7		0.3	0.9	4.2	3.4	8.8
	7.4		4.9	1.3	4.6	9.2	4.9	7.0	6.6	9.4	5.0	9.9	5.3	5.9		6.7	4.8	5.3	7.4	8.0	6.1	6.1	3.9	6.5	4.1	5.4	7.6	
	6.6	3.4	4.6	6.4	7.7	10.0		8.2	9.7	5.5	8.9	4.9	6.6		8.7	6.0	1.1	7.3	5.7	6.9		10.0	7.7	9.2	10.0	9.6		9.9
	8.7	5.2	2.8	0.2	6.1		4.8	8.4	5.7	3.9	0.5		8.3	2.9	8.5	6.1	5.7	7.7		9.2	9.9	10.0	9.3	5.2		9.0	10.0	9.1
	8.3	8.3	10.0		7.0	9.2	7.2	6.8	6.1	3.8		7.0	4.9	5.3	8.1	7.5	5.3		6.5	10.0	10.0	10 0		10 0	4.9	3.3	2.4	9.1
	4.6		4.9	10.0	9.8	6.4	8.3	0.2			5.8	0.6	10.0	9.2	8.8		2.5	5.3	6.2	2.1	5.8	8.8	5 3	6.5	4.2	7.0	8.5	7.6
1843		4.8	8.7	8.3	10.0	10.0	9.7	9.7	10.0	8.3	10.0	9.9	7.2		5.2	7.1	5.1	9.4	7.2	7.0		7.4	10.0	10.0	6.0	10 0		0.1
	7.0	8.2	9.6	3.7	10.0		10.0	6.9	7.3	7.2	9.8	10.0		10.0	10.0	7.2	4.7	3.8	6.7		7.5	0.3	9.2	9.9	9.7		5.9	9.6
	10.0	8.7	2.2	0.5		5.2	6.3	8.2	6.0	1.9		9.9	5.4	2.2	8.1	9.3	9.7		9.3	10 0	2.9	8.7	4.5		6.1	2.2	4.9	5.8
		10.0		7.2	2.1	6.2	4.1	4.4		2.7	6.1	6.1	6.8	9.3	9.4		0.1	0.1	5.7	6.3	7.5	7.9		8.9	8.3	6.8	8.4	5.9
	6.8		9.2	9.6	10.0	9.3	9.7	8.1	6.7	9.5	8.5	4.7	6.5	7.8		7.4	6.5	9.8	8.7	8.9	6.4		4.3	6.2	9.6	7.3	8.4	9.1
		9.6	10.0	10.0	4 5	2.9	5.0		5.3	6.9	0.8	6.1		7.0	5.9	8.2	7.6	9.8	7.3		5.2	8.0	4.8	9 3	6.1	7.8		6.3
	10.0	9.4	9.4	7.8	5.7		5.0	8.0	5.9	10.0	8.7		5.5	7.5	7.2	8.1	8.8	8.1		7.7	3.4	6.9	6.4	7.0	5.8		5.4	8.7
	7.7	6.0	1.6	3.1		6.2	8.7	7.2	2.6		4.0	7.3	6.0	6.9	7.0	5.4		9.1	9.7	9.4	5.4	5.2	2.4		5 3	0.1	1.3	1 0
	5.5	5.6		8.1	6.6	0.8	8.8	5.3		5.1	6.4	1.4	3.8	1.8	5.1		7.2	6.8	5.5	3.4	3.3	9.7		8.3	8.8	6.9	6.1	9.8
	7.9		7.9	6.6	9.9	6.7	4.4	5.3	2.0	8.4	2.8	0.6	3.8	6.7	7.8	3.7	9.0	7.2	2.3	4.1	5.3		10.0	8.3	10.0	7.7	8.1	
	9.6	7.0	4.2	2.7	9.9	4.9		5.5	6.3	4.7	8.4	3.7		2.9	9.8	9.9	8.9	8.7	9.7		7.1	7.8	6.4	10.0	7.2		10.0	10.0
	1.3	8.9	4.4	8.6		4.8	10.0	10.0	5.5	7.9	7.3		9.2	10.0	10.0	9.1	9.8	9.8			10.0	10.0	10.0		6.2	4.6	6.3	9.8
1844	10.0	5.4		6.2	9.3	8.4	8.7	8.7	9.8		6.2	3.9	9.1	10.0	7.4	7.7		7.3	9.8	6.6	6.3	5.7		6.4	6.8	7.6	1.3	10.0
	8.3		3.6	6.8	9.8	1.3	6.5	8.0		8.4	9.4	9.7	9.4	1.2	8.4		6.8	3 6	8.3	7.3	5.3	8.0	4.7	6.2	6.1	7.5	7.2	
	8.9	8.3	3.5	8.4	6.5	7.9		8.7	4.3	3.0	9.5	8.8	9.0		6.1	9.6	7.7	2.3	9.8	7.8		9.2	10.0	4.5	2.7	4.4		4.7
	1.0	0.0	2.1		3.4		3.0	0.9	4.9	8.4	9.8		5.5	7.7	4.2	4.2	5.8	9.5		5.5	1.6	3.9	3.8	4.0		1 3	2.5	1.0
	0.2	4.0	9.8		3.2	4.1	7.6	5.3	7.8	7.8		3.7	4.3	6.6	9.3	6.0	5.1		3.6	7.6	8.5	7.3	5.7		6.0	8.7	10.0	9.3
	9.6	2.9		6.2	4.9	6.6	9.0	8.0		4.9	2.7	3.5	5.5	3.0	4.4		4.8	9.4	10.0	8.5	5.3	4 0		5.5	10.0	10.0	9.1	9.9
	4.5		8.4	9.8	8.9	9.7	9.9	9.4	8.3	6.4	7.6	8.7	9.4		2.5	6.9	7.5	6.2	6.9	2.5		0.6	2.4	4.0	5.3	8.4	6 2	
	5.9	8.8	7.5	5.2	4.8	9.4		7.8	3.1	3.1	4.3	8.3		9.7	8.8	7.8	7.8	5.7	6.0		3.6	8.6	8.4	9.5	6.8	6.9		10 0
	8.7	1.8	0.2	3.6	0.4		2.1	7.2	5.8	7.8		10.0	8.8	4.0	8.5	9.5	9.1		7.8	8.5	8.8	2.7	5.3	6.0		9.8	5.9	3.1
	0 3	0.4	0.0		1.8	5.9	8.8	8.0	8 5		3.5	4.4	9 6	6.4	2.2	8.1		8.0	6.1	7.2	7.3	2.4	5.8		10.0	5.9	4.7	10.0
	10.0	7.6		3.9	10.0	9.8	1.8	7.8	10.0	7.6	10.0	7.6	8.4	9.0	5.1		8.1	10.0	8.3	9.4	9.8	7.3	10.0	10.0	7.5	3.4	10.0	8.5
		9.7	2.4	2.0	7.3	9.8	9.1		10.0	7.8	0.2	0.4	6.3		10.0	10.0	9.2	10.0	10.0	10.0		10.0	10.0	10.0	3.6	9.8		10.0
	10.0		10.0	9.5	8.9		9.2	9.1	9.3	9.2	8.8	9.5		8.6	8.8	10.0	10.0	8.3	10.0		9.1	9.8	9.7	8.8	8.0	7.0	2.1	7.0
1845	10.0	8.7	7.0		7.6	5.9	9.8	8.5	9.1	5.9		9.2	7.6	5.6	1.2	2.8	5.7		10.0	3.6	2.8	9.8	7.3	8.0		8.7	7.5	1.8
	2.7	9.4		8.5	9.5	7.7	9.8	8.3	8.4		8.3	6.5	7.0	8.3	7.5	5.4		10.0	6.7	6.7	7.1	0.0	4.6	6.0	8.3	3 4	10.0	6.7
		9.9	7.4	10.0	5.9	2.2	0.5			5.3	2.9	1.0	1.6	4.2		0.0	4.4	7.5	10.0	9.3	10.0		10.0	8.4	2.5	5.8	4.2	
	1.0	4.3	0.5	5.8	7.3	6.7		9.2	9.6	10.0	6.5	5.2	7.7		8.0	9.2	9.6	9.9	7.8	3.9		8.3	9.6	10.0	8.0		8.3	8.4
	9.6	6.3	9.9	10.0		8.0	4.2	9.0	10.0	6.0	6.5		5.1	5.6	6.8	8.3	6.8	8.1		3.5	0.5	0.0	3.6	4.2		8.8	8.5	7.3
	5.1	2.7	3.0		4.9	4.1	8.5	5.3	8.6		9.4	7.9	7.5	4.3	7.9	5.3		3.3	6.4	8.9	9.2	9.8	9.0		8.9	9.1	5.7	9.1
	7.3	2.9		7.4	6.8	10.0	10.0	10.0		7.5	6.1	6.4	3.1	5.2	9.4		8.4	8.5	7.1	7.1	6.6	8.5	9.0	9.8	9.1	9.3	7.4	8.1
		6.7	9.5	3.5	2.8	1.5	7.6		3.2	5.9	6.6	2.3	1.8		7.9	9.5	10.0	8.1	9.7	3.9		2.9	0.0	6.8	10.0	3.6	4.8	
	8.3	9.3	8.0	6.7	1.4	5.7		5.8	0.8	8.1	2.6	8.3		7.8	7.7	5.8	9.8	8.4	5.6		6.8	7.2	8.0	5.8	5.0	6.7		4.0
	0.0	5.9	5.8	9.9	7.8		5.2	3.3	4.4	3.3		9.2	7.2	4.5	6.4	9.3	2.6		0.8	0.0	3.7	8.6	7.9	9.6		6.5	7.0	6.8
	6.0	8.7	7.7		6.2	7.4	6.8	4.5	4.6		5.5	7.5	10.0	9.7	9.6	8.5		6.2	6.4	7.1	4.3	3.8		5.6	4.8	4 3	6.5	3.5
	5.8		9.3	9.3	10.0	10.0	8.0	4.9	9.9	6.6	1.3		9.3	5.9		5.7	6.2	6.2	4.5	2.8	8.0		9.1	9.6	10.0	10.0	10.0	
1846	10.0	7.1	8.1	8.2	9.6	8.6		9.9	3 8	10.0	7.3	7.5	7.6		8.5	7.6	6.8	8.0	7.3	8.3		9.3	4.6	9.5	5 9	6.3		4.5
	6.8	4.8	8.7	9.8	7.4		9.9	9.9	10.0	10.0	7.3		9.0	7.1	7.6	5.4	7.1	4.6		6.4	8.9	8.3	6.8	7.8		4.0	3.3	6.8
	8.3	6.7	10.0		9.7	5.8	8 3	8 3	9.3	6.4		5.2	5.3	6.7	5.5	3.3	8.1		5.8	5.2	7.3	9.0		9.4	9.9	8.9	5.6	
	9.5		6.3	7.2	10.0	7.3	9.6	10.0		6.6	6.4	6.3	6.6	7.7	9.0		4.7	7.3	3.3	6.6	10.0	9.6	9.1	8.3	8.6	6.9	8.5	6 8
		3.5	3.1	9.9	3.2	1.9	7.8		9.2	7.1	6.9	6.8	4.6	8.4		4.8	8.0	5.7	4.2	5.9	0.8		0.0	1.6	4.5	5.2		8.2
	5.2	8.7	7.7	5.4	2.8		1.2	2.5	0.4	1.4	6.3	6.0		6.5	9.3	8.1	6.3	9.0	6.4		3.5	4.6	9.8	5 8	0.0		8.5	10.0
	9.9	9.8	8.0	6.8		3.0	4.8	4.3	10.0	7.3	8.3		7.5	9.4	7.4	4.7	8.6	3.0		9.8	8.1	2.3	5.5	7.7		8.3	7.3	7 0
	8.1	8.3	8.3		9.9	8.3	7.2	7.4	5.6		7.5	7.5	9.0	9.8	7.2	6.9		9.6	8.3	8.7	6.5	5.6		5.1	8.8	3.5	0 0	1 3
	3.7		5.1	6.8	6.4	8.7	6.6	8.3	8.1	5.8	3.8	5.0	3.5	3.7		9.1	6.3	7.3	9.2	6.5	7.4		8.5	2.6	6.6	6.8	10.0	5.8
		8.4	9.3	8.7	5.3	9.2	6.3		8.1	8.5	6.5	7.1	7.1		2.7	7.3	8.8	9.7	5.5	7.4		9.4	9.2	10.0	10.0	9.1	10.0	
	7.0	6.9	4.5	8 9	10.0	10.0		9.1	7.7	10.0	10.0		9.3	9.8	5.2	5.3	5.6	4.4		9.8	9.2	8.1	4.0	9.9	6.0		5.8	7.5
	9.8	6.8	2.9	6.3		9.7	8.8	9.9	7.6		1.7	3.2	1.9	6.3	3.9	10.0		7.5	6.1	10.0	9.9		0.3		9.8	8.9	9.1	3.1
1847	6.1		9.0	8.8	10.0	9.1	9.2	6.8	6.5	5.8	4.3	7.3	5.6	7.3		7.8	6.7	4.8	10.0	4.8	8.8			6.9	8.3	7.6	6.3	5.6
	9.8	10.0	6.6	10.0	10.0	10.0		9 2	6.9	2.8	6.5	8.8		0.2	1.0	0.2	0.0	0.9	8.4		5.6	4.6	8.6	1.8	8 0	6.1	8.4	
	5.0	9.1		9.0		7.6	8.7	8.0	7.4	5.7	4.1		9.5	10.0	7.2	7.8	5.9	5.2		6.7	5.0	7.0	2.1	7.2	5.8		6.6	4.8
	7.8	6.0		8.4	8.3	7.5	6.2	8.3	9.7		7.7	8.3	5.0	6.4	5.1	7.8		7.6	9.8	6.8	4.7	3.1	2.6		8.6	7.1	4.6	7.2
	6.8		1.9	0.0	0.6	3.8	6.3	9.2		7.3	8.0	6.8	10.0	3.7	3.8		9.3	4.6	9.2	8.9	9.0	6.2		8.9	4.1	0.0	1.4	2.3
	8.3	9.5	6.2	7.4	9.8	6.6		2.0	2.0	0.8	7.8	6.8	7.7	8.0		2.3	2.7	5.3	4.1	3.6	10.0		5.8	6.6	8.4	9.3	7.2	
	2.5	8.0	5.1	5.2	1.2		3.9	3.5	5.0	10.0	8.3	6.4		8.0	9.2	5.8	7.5	6.8	3.1		10.0	10.0	7.4	6.7	3.3	5.7		4.2
	7.7	6.6	6.5	5.8		7.9	7.1	5.0	7.3	4.5		7.3	6.1	8.5	5.2	3.9	4.5		10.0	4.3	7.6	7.2	9.6	6.7	0.8		9.0	9.0
	8.3	5.1		1.5	7.6	7.2	9.2	9.8	10.0		9.6	7.8	5.1	6.2	4.3	10.0		6.8	6.5	8.7	10 0	8.5	5.3		8.4	9.0	6.8	9.0
	10.0		4.6	5.9	9.3	10.0	8.6	9.0	5.8	1.7	10.0	10.0	7.8	9.3		7.3	4.8	0.7	8.8	9.9	6.9			8.7	4.5	4 6	5.6	3.9
		8.4	6.2	5.0	9.8	10.0	10.0		3.5	7.8	10.0	7.4		6.7	9.6	3.3	9.0	9.2	7.2		6.3	5.1	7.5	8.0	5.3	1.4	3.6	9.2
Means,	6.8	6.7	6.1	6.9	7.0	6.9	6.9	6.9	6.6	6.5	6.3	6.6	6.6	6.4	6.8	6.6	6.6	6.6	6.8	6.5	6.4	6.7	6.6	7.0	6.6	6.5	6.8	7.0

Average degree of cloudiness from seven years' observations, 6.7.

Average Cloudiness.		Average Cloudiness.		Average Cloudiness.		Average Cloudiness.	
Full Moon	6.8	Third quarter	6.9	New Moon	6.8	First quarter	6.7
1st day after do.	6.7	6th day before New	6.6	1st day after do.	6.6	6th day before Full	6.6
2d " "	6.1	5th " "	6.5	2d " "	6.6	5th " "	7.0
3d " "	6.9	4th " "	6.3	3d " "	6.6	4th " "	6.6
4th " "	7.0	3d " "	6.6	4th " "	6.8	3d " "	6.5
5th " "	6.9	2d " "	6.6	5th " "	6.5	2d " "	6.8
6th " "	6.9	1st " "	6.4	6th " "	6.4	1st " "	7.0

These numbers indicate but slight deviations from 6.7, which is the average cloudiness of the whole period (10 representing a sky perfectly overcast). In other words, exactly two thirds of the sky at Greenwich is, upon an average, covered with clouds. The greatest departure from this mean occurs on the second day after full, when the average is 6.1. This might be suspected to indicate a law of nature; but such a conclusion is discountenanced by the fact that the amount of cloudiness in the different years was very unequal. In 1844, the average cloudiness for the second day after full was only 4.7, which is 2.0 below the average result for the seven years. In 1842 the average for the second day after full was 5.7; or 1.0 below the general mean. The average of the remaining five years is 6.5, which is only 0.2 below the general mean for the entire period.

If we divide the whole month into four parts, in such a manner that the middle of the intervals shall correspond to new moon, first quarter, full moon, and last quarter, we shall obtain the average cloudiness at new moon, 6.6; first quarter, 6.7; full moon, 6.7; and last quarter, 6.7; results which may be pronounced entirely identical, and seem to demonstrate that Sir John Herschel's meteorological *fact* is unmingled *moonshine.*

It may possibly be objected, that my mode of discussing these observations is calculated to conceal the fact claimed by Sir John Herschel, inasmuch as I have employed the average cloudiness of each entire day, whereas the full moon is only claimed to exert an influence when she is above the horizon. I do not admit the force of this objection; for if the full moon, when above the horizon, exerts an influence to dissipate the clouds, such an influence ought to appear in the average cloudiness of the twenty-four hours. This conclusion can only be avoided by supposing that the full moon, when below the horizon, exerts a positive influence on the clouds, contrary to what is produced when she is above the horizon, and the one influence ought

to be just as palpable as the other, and ought not to have escaped the notice of so shrewd an observer as Sir John Herschel. In order, however, not to leave any room for cavil on this point, I have compared the observations made at midnight (Göttingen time) on the days of new moon, first quarter, full moon, and last quarter, for the entire period of seven years. The following table exhibits the individual observations: —

Year.	New Moon.	First Quarter.	Full Moon.	Last Quarter.	Year.	New Moon.	First Quarter.	Full Moon.	Last Quarter.
1840	10	10	10	10		0	10	0¼	10
	9½	0	4	4		10	1	8	10
1841	10	10	0	10		0	0	0	1
	10	8	10	10		5	3	10	10
	8	10	9½	0		10	10	10	10
	8	8	8	10	1845	10	10	10	10
	0	10	1	0		0	10	10	10
	10	9½	0	10		0	10	1	2
	5	9	4	9½		0	10	10	0
	7	9½	3	0		0	0½	10	10
	10	10	10	0		10	0	10	1
	10	0	1	10		10	10	9	7
	0	10	10	0½		10	10	6	10
	8	10	10	10		10	0	4	0
1842	10	10	9½	2		10	9¾	0	0
	10	10	10	10	1846	7	10	0	10
	10	10	10	10		10	5	10	10
	10	10	8	8		0	0	9	9
	0	0	2	0		2	10	10	0
	0	1	9	0		0	0	9½	10
	10	1	9	10		0	10	0	4
	9	0	10	7		10	10	4	10
	10	10	0	0		10	0	0	5
	10	10	5	10		9	10	1	10
	0	10	0	9		9	2	10	0
	10	10	9	2		9½	9	10	9
	6	0	0	0		7	10	9	9
1843	3	10	0	10		10	10	10	6
	10	10	10	10		9½	1	8	1
	10	10	10	10		9¾	10	10	0
	10	0	10	10	1847	10	10	10	10
	10	3	2	10		0	10	10	8
	10	5	10	10		0	0	8	10
	10	0½	10	4		10	10	10	4
	5	0	3	0		0	8	0	10
	0	10	10	0		10	4	7	10
	10	6	10	2		1	10	10	0
	9½	10	8	10		8	10	4	0
	10	10	2	5		2	3	10	0
1844	8	10	10	10		10	10	0	10
	2	10	10	10		10	0	10	10
	10	10	10	4		6	0	10	0
	0	0	0	2		. .	. .	10	10
	10	10	0	9					
	5	9	10	1	Means	6.7	6.7	6.6	6.1
	0	0	7	10					

According to these observations, the average cloudiness at midnight on the day of new moon is 6.7; at the first quarter, 6.7; at full moon, 6.6; and at the last quarter, 6.1. The first three numbers may be pronounced identical with the average cloudiness of the entire period, as already explained; the last might be suspected as indicating a general law; but if we compare the results for each year separately, we shall find that they range from 5.0 to 8.0, showing that causes which are independent of the moon's age exert a powerful influence upon the degree of cloudiness. Moreover, we see that in 1841 and 1842 the sky was remarkably clear at the last quarter, while the average of the remaining years is 6.5, almost identical with the results of the other periods of the moon.

This comparison, therefore, leads us to the same result as the former, namely, that the Greenwich observations, which furnish the degree of cloudiness of the sky every two hours, night and day, for seven years, give no countenance to the fact claimed by Sir John Herschel; and we have another example of the danger of drawing general conclusions from observations loosely treasured up in memory, without testing them by reference to recorded tables.

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4. An Investigation of the Storm Curve, deduced from the Relation existing between the Direction of the Wind, and the Rise and Fall of the Barometer. By Professor James H. Coffin, of Lafayette College, Pa.

At our meeting in New Haven, under our old organization, in 1845, I read a paper, in which I pointed out a law that I had discovered to exist between the direction of the wind and the rise and fall of the barometer, and hinted that the subject might throw some light on the much debated question of the law of storms, and also intimated my purpose to prosecute the investigation further at some future time. The data on this subject which I used in preparing that paper were derived from observations taken at St. John's, Newfoundland; Ogdensburg, New York; Boston and Nantucket, Mass.; Philadelphia, Pa.; Greenwich, England; and the Bermuda Islands;—

only seven in all, and some of them very imperfect. I was not then aware that the subject had ever been at all investigated, except in the very loose and deceptive way that we find it treated of in some of our Encyclopædias, and from which one would form ideas anything but correct in regard to the law in question. I subsequently found, however, that in 1837, one year before I commenced my investigation for a single locality,* Professor Dove, of Berlin, had instituted a similar one, and had published his results, coinciding exactly with mine, for three places in Europe, and from observations taken during two or three voyages at sea. I have also since obtained data, more or less perfect, from one station in Russian America, one in Iceland, one among the Ural Mountains, one in Siberia, one in China, and one more in the United States; so that I am now able to present, in the following tables, the results at eighteen independent and widely remote stations,† situated in Europe, Asia, and America, and on the Atlantic and Pacific Oceans, — a number fully sufficient, if they harmonize, to establish a general law.

The figures in the several columns show, in decimals of an inch, the average rise or fall of the barometer, for a period of 24 hours, during winds from the points of compass set opposite them, the sign + denoting a rise, and the sign — a fall.

At Ogdensburg, the exact duration of each wind was recorded by a self-registering vane, and the changes in the barometer, noted at intervals of about eight hours, were distributed among the winds that had blown during these intervals, according to their duration. At all the other stations, the change in the barometer during each interval between the times of observation was divided equally between the winds that blew at the beginning and end of it. The length of these intervals differed at different places. At Sitka, and probably at Bermuda, the observations were taken hourly through the entire twenty-four hours of the day. So, also, for the most part, were the observations taken at sea.‡ At Girard College, Bogoslowsk, Barnoule, and

* See Fifty-second Annual Report of the Regents of the University of the State of New York, pp. 223 – 226.

† Including those of Professor Dove.

‡ The numbers in the columns of Table I., which show the results at sea, are not the real means for the several points, but these means combined with those for the two contiguous points on either side. This was done by Professor Dove "in

Pekin, they were taken bi-hourly; those at Girard College being kept up day and night, those at Pekin from 5 o'clock A. M. till 11 P. M., and those at Bogoslowsk and Barnoule from 8 A. M. till 10 P. M. At Iceland, the observations were taken, sometimes thrice, sometimes twice, and sometimes only once a day. In using the observations reported by the Franklin Institute, only one interval per day is taken into account, namely, between two observations of the barometer, one taken at sunrise, and the other at two o'clock P. M. The direction of the wind was usually recorded but once a day.

In the absence of the original records, which have passed out of my hands, I am unable to give the intervals at which observations were taken at Newfoundland, Boston, Nantucket, and Greenwich. At the latter place they were, probably, taken twelve to sixteen times a day, and at the others not more than two or three. In Professor Dove's article, he states that "the variations" (i. e. of the barometer) "in Paris are calculated for twelve hours"; those at Dantzic, sixteen; and those at London, "from morning till evening, without further specification of the hours of observation."

Instead of wearying the Association with a rehearsal of the following table in detail, the results may be exhibited in a more striking and satisfactory manner, by the aid of the set of roughly sketched barometrical wind-roses, herewith presented,* in which the width of the shading at the several points of compass is proportional to the average rise or fall of the barometer per day while the wind was from those points, the darker shading denoting a rise, and the lighter a fall. In order to make the shading more symmetrical in several of the wind-roses, without affecting the principle of the illustration, the mean rise or fall for each point of compass, as given in the table, is combined with the two contiguous ones on either side, and the shading made proportional to the new means thus found.

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order to compensate for the too rare occurrence of some directions of the wind for the exposition of the law." I have done the same with the observations at Boston, Bermuda, and Iceland.

* Not received.

TABLE I.

Showing the Average Rate of Rise or Fall of the Barometer, in Decimals of an Inch, per twenty-four Hours, during Winds from different Points of Compass.*

Course.	Boston, 4 Months.	Franklin Inst., Phil., 1839, 1841, and 1842, in part.	Iceland, June 1, 1811, to June 1, 1812.	London, 3 years.†	Bogoslowsk, Ural Mts., Jan. 1 to Aug. 1, 1838.	Pekin, China, April and May, 1842.	Barnoule, Siberia, Jan. and Feb. 1838.‡	Sitka, Rus. America, April, 1842.
N.	+.014	+.021	+.131	+.098	+.055	+.174	—.133	—.280
N.E.	—.003	+.003	—.063	+.036	—.016	—.052	—.147	—.260
E.	—.025	—.099	—.169	—.024	—.013	—.225	—.004	—.074
S.E.	—.109	—.162	—.235	—.098	—.064	—.191	—.085	+.032
S.	—.083	—.171	—.175	—.096	—.078	—.133	+.026	+.189
S.W.	—.057	—.105	—.043	—.049	—.005	—.043	+.094	+.185
W.	+.006	+.042	+.102	+.022	+.022	+.080	+.122	+.077
N.W.	+.010	+.084	+.125	+.064	+.076	+.102	+.149	+.015

Course.	Ogdensburg, N. Y., One Year.	Girard College, Phil., June 12, 1840, to May 31, 1841.	Bermuda, 1840, 1841, and 1843, in part.
North . . .	+.080	+.160	+.095
N. by E. . .	+.095	+.141	+.027
N. N. E. . .	+.016	+.085	—.003
N. E. by N. .	—.041	—.026	—.025
N. E.	—.105	—.064	—.014
N. E. by E. .	—.139	—.137	—.021
E. N. E. . .	—.183	—.218	—.013
E. by N. . .	—.173	—.158	—.025
East	—.149	—.303	—.033
E. by S. . .	—.146	—.346	—.069
E. S. E. . .	—.122	—.130	—.059
S. E. by E. .	—.097	—.635	—.047
S. E.	—.123	—.184	—.056
S. E. by S. .	—.155	—.111	—.075
S. S. E. . .	—.156	—.244	—.126
S. by E. . .	—.144	—.191	—.105
South . . .	—.178	—.186	—.088
S. by W. . .	—.131	—.074	—.032
S. S. W. . .	—.087	—.164	—.023
S. W. by S. .	—.034	—.100	—.028
S. W. . . .	+.014	—.090	—.026
S. W. by W. .	+.060	—.019	—.020
W. S. W. . .	+.066	—.024	—.006
W. by S. . .	+.137	+.064	+.015
West	+.125	+.100	+.078
W. by N. . .	+.155	+.171	+.103
W. N. W. . .	+.219	+.263	+.103
N. W. by W. .	+.250	+.159	+.080
N. W. . . .	+.266	+.184	+.068
N. W. by N. .	+.219	+.208	+.092
N. N. W. . .	+.192	+.198	+.121
N. by W. . .	+.193	+.110	+.126

* These calculations were commenced without any view to publication, and, in a few cases, nothing was preserved but the result; so that it is impossible now, after the lapse of years, to say with certainty whether the variations were computed for twenty-four hours, or for a longer or shorter time. This does not, however, affect the question of the relative influence of the different winds, since it is certain that all for the same place were computed for the same time, whatever that time may be.

† Dove.

‡ The East and West points were computed for the first nine months of the year.

TABLE I. — *Continued.*

Course.	Newfoundland.	Nantucket, 1838, 1840, and 1841, in part.	North Atlantic Ocean,* 20 days.	Greenwich, England, 9 years.	Paris, France, 10 years.*	Dantzic, Prussia, 15 years.*	At Sea, in the Southern Hemisphere,* 8 months.
North	+.337	+.165	+.088	+.237	+.020	+.050	—.037½
N. N. E.	+.156	+.060	—.048	+.159	—.011	+.010	—.035
N. E.	+.080	+.033	—.095	+.042	—.015	+.041	—.023
E. N. E.	—.195	—.251	—.097	—.126	—.076	—.013	—.017
East	—.207	—.190	—.084	—.268	—.084	—.010	—.004½
E. S. E.	—.420	—.361	—.071	—.312	—.092	—.003	+.001
S. E.	—.283	—.254	—.066	—.249	—.076	—.016	+.009
S. S. E.	—.458	—.262	—.082	—.500	—.076	—.051	+.024
South	—.320	—.174	—.122	—.395	—.074	—.069	+.045
S. S. W.	—.178	—.141	—.117	—.169	—.074	—.067	+.064½
S. W.	+.060	—.085	—.047	—.103	—.014	—.012	+.073
W. S. W.	+.097	+.012	+.031	+.037	+.004	+.021	+.037½
West	+.111	+.122	+.088	+.074	+.066	+.008	—.010
W. N. W.	+.304	+.172	+.141	+.259	+.090	+.064	—.032
N. W.	+.289	+.186	+.211	+.226	+.076	+.065	—.035
N. N. W.	+.175	+.231	+.210	+.075	+.090	+.088	—.035½

Regarding the rate of rise or fall in the barometer during winds from each point of compass, given in the preceding table, as the measure of the force that produces it, and reducing these forces to a single force in the usual way, we obtain the results in the second, third, and fourth columns of the following table; to which I have added, in the fifth column, the mean direction of the wind.† The arrows within the inner circle of the wind-roses exhibit these results to the eye.

TABLE II. *Points of Maximum and Minimum Pressure.*

Point of Observation.	Point of Maximum Pressure.	Point of Minimum Pressure.	Mean line of Maximum and Minimum Pressure.	Mean Direction of Wind.
	° ′	° ′	° ′ ° ′	° ′
Ogdensburg	N. 51 2 W.	S. 58 14 E.	N. 54 17 W. to S. 54 17 E.	S. 58 34 W.
Newfoundland	N. 35 50 W.	S. 42 12 E.	N. 39 31 W. to S. 39 31 E.	S. 78 4 W.
Girard College	N. 4 4 W.	S. 53 12 E.	N. 44 57 W. to S. 44 57 E.	N. 74 5 W.
Franklin Inst.	N. 50 16 W.	S. 21 10 E.	N. 28 31 W. to S. 28 31 E.	S. 75 4 W.
Boston	N. 28 21 W.	S. 14 39 E.	N. 18 56 W. to S. 18 56 E.	N. 88 20 W.
Nantucket	N. 35 37 W.	S. 48 3 E.	N. 42 36 W. to S. 42 36 E.	N. 77 0 W.
Bermuda	N. 41 32 W.	S. 36 19 E.	N. 39 22 W. to S. 39 22 E.	S. 45 48 W.
North Atlantic	N. 54 49 W.	S. 51 31 E.	N. 53 17 W. to S. 53 17 E.	S. 83 25 W.†
Iceland	N. 39 18 W.	S. 48 48 E.	N. 45 11 W. to S. 45 11 E.	N. 86 35 W.
London	N. 13 55 W.	S. 17 4 E.	N. 15 38 W. to S. 15 38 E.	N. 88 38 W.
Greenwich	N. 34 6 W.	S. 34 4 E.	N. 34 5 W. to S. 34 5 E.	S. 60 14 W.
Paris	N. 51 34 W.	S. 48 48 E.	N. 50 0 W. to S. 50 0 E.	S. 70 30 W.
Dantzic	N. 29 48 W.	S. 6 37 E.	N. 20 5 W. to S. 20 5 E.	S. 68 7 W.
Ural Mountains	N. 34 51 W.	S. 29 46 E.	N. 32 18 W. to S. 32 18 E.	N. 83 21 W.
Barnoule	N. 87 11 W.	N. 43 49 E.	S. 70 19 W. to N. 70 19 E.	S. 35 3 W.
Pekin	N. 31 47 W.	S. 54 34 E.	N. 45 10 W. to S. 45 10 E.	S. 74 22 W.
Russian Amer.	S. 30 15 W.	N. 29 16 E.	S. 29 41 W. to N. 29 41 E.	S. 55 37 E.
S. Hemisphere	S. 25 21 W.	N. 9 53 W.	S. 10 22 W. to N. 10 22 E.	N. 83 44 W.†

* Dove.

† The observations at sea were taken in various latitudes, and those on the direc-

The results shown in the foregoing tables and diagrams confirm all that I adduced in my former paper, and establish conclusively, I think, the following facts, at least in the zones of westerly winds.

1st. That the horizon is divided by nature into two well-defined portions, the winds from between the division points on the one side being all attended with a rise in the barometer, and on the other with a fall. This is found true at all the stations where there are reliable observations. Even where they are taken for thirty-two points of the compass, there is no intermingling.

2d. That, in the northern hemisphere, one of these points lies in a southwesterly direction, and the other in a northeasterly. Barnoule in Siberia, and Sitka in Russian America, look like exceptions; but at both these places the results were computed for a short time only,* and might be somewhat modified by making use of a longer series of observations. It is probable, moreover, as I have shown elsewhere, that Sitka lies without the zone of westerly winds, and where a different law may prevail.

3d. That, in the northern hemisphere, a wind arriving from its mean direction always finds the point of maximum pressure on its left, and the minimum on its right; while the reverse is true in the southern hemisphere. There appear to be no exceptions to this law.

4th. That the line of its approach generally makes an angle, more or less acute, with one drawn to the point of maximum pressure. The only exception is at Hamilton,† our station at the Bermudas, where it is slightly obtuse (92° 40′). Nor is the result different, if, instead of regarding the mean resultant of all the forces which raise the barometer as the point of maximum pressure, we (perhaps more properly) regard each fall as a negative rise, and *vice versa*, and then obtain one

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tion of the wind not reported; so that it was impossible to know accurately what mean direction to assign. But taking into account the circumstances of the voyages during which they were taken, I have assumed, as approximately correct for the southern hemisphere, one that I computed from a zone on Lieutenant Maury's charts, extending from lat. 40° to 45° S., and from long. 20° E. to 120° W.; and for the North Atlantic, one deduced from about twelve years' observations, taken north of lat. 36°.

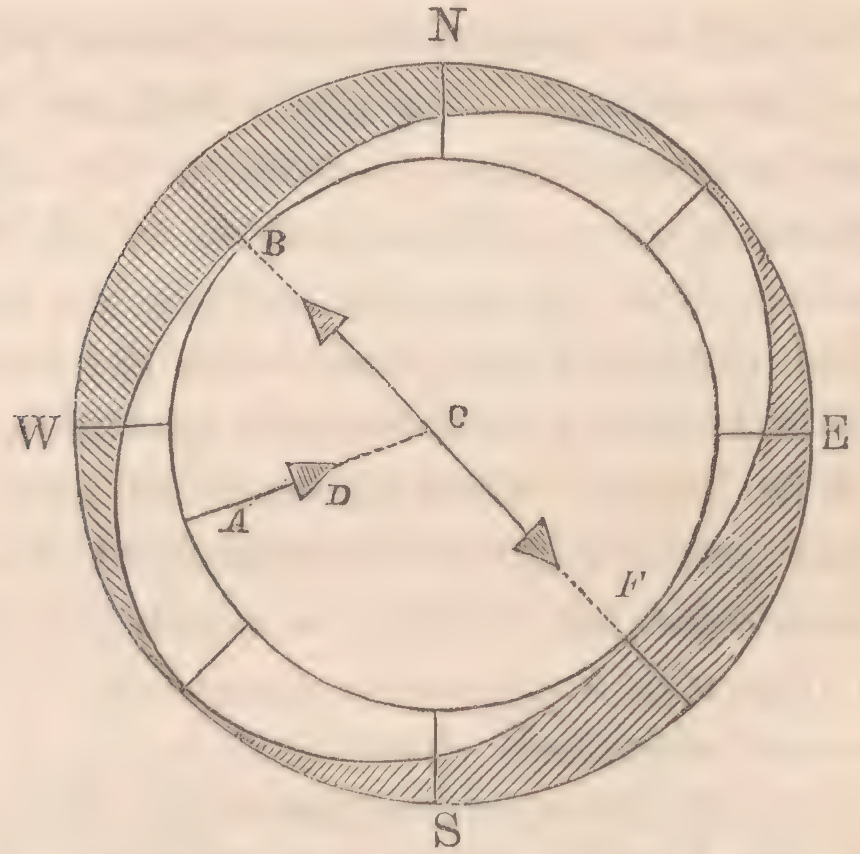
* One month at Sitka and two at Barnoule.

† It is worthy of remark that here, too, the angle is acute, if, instead of the mean direction of the wind observed at Hamilton, we employ that at Ireland Isle, another island in the same group, or even the mean between the two.

mean resultant for the whole. The fourth column in Table II. was computed in this way, and the results are shown on the wind-roses by a broken line.

Such uniformity in these results bespeaks some general law or system of movement in the atmosphere that shall harmonize with them ; and it requires but a slight examination to perceive that they will be completely satisfied and accounted for by supposing that in the general current of the atmosphere there are occasional eddies ("cyclones"), in which the air revolves spirally from right to left in the northern hemisphere, and from left to right in the southern, the curve ever making an angle with the radius vector equal to that which the mean direction of the wind makes with the maximum and minimum line of the barometer, and that the barometer falls in the forward half of these eddies and rises in the latter half, the amount of rise or fall diminishing as we recede from the central axis on either side. For example, let us take the barometrical wind-rose, N E S W, Fig. 1,*

Fig. 1.



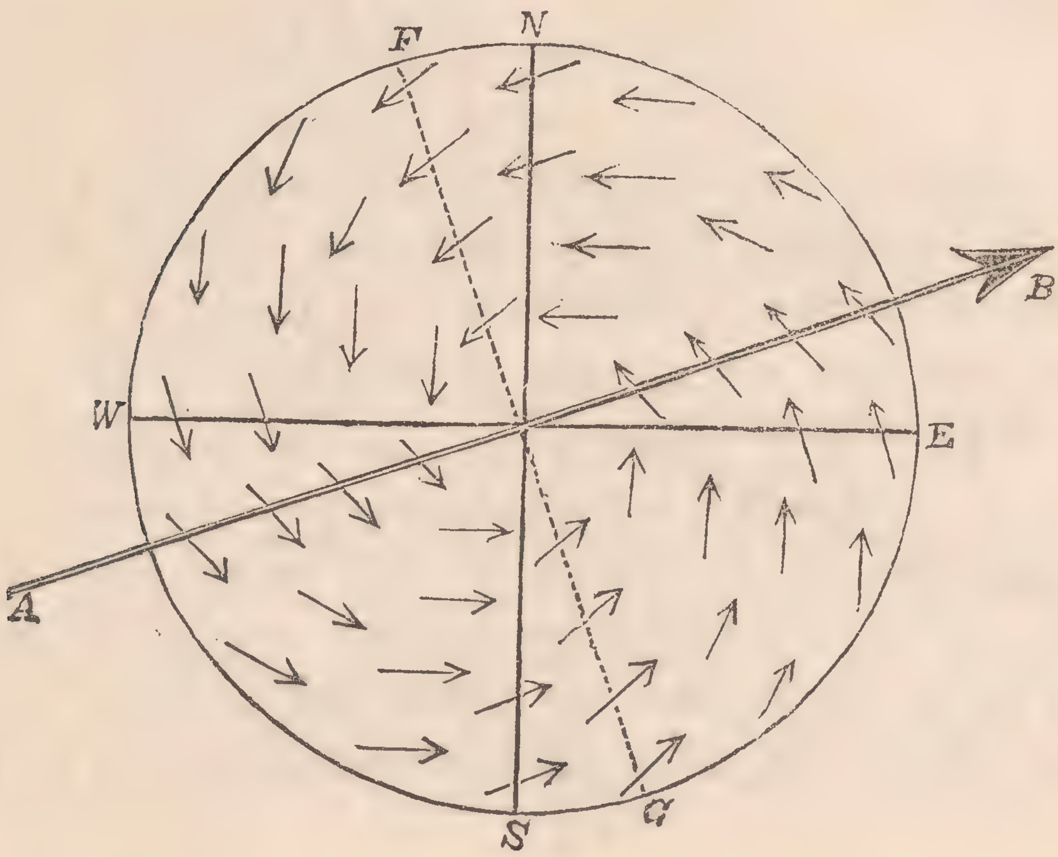
in which the mean direction of the wind, *A D*, is assumed to be S. 70° W., and the mean line of maximum and minimum pressure, *B F*, from N. 45° W. to S. 45° E., making the angle *A C B* 65°.

Let now *A B* (Fig. 2) represent the direction of the mean atmospheric current as above, namely, from S. 70° W., and *N E S W*

* For convenience of illustration, this wind-rose is drawn perfectly symmetrical, which is only approximately true of those drawn from actual observation.

one of these eddies, in which the direction of the local wind makes a constant angle of 65° with each radius that it passes. By drawing

Fig. 2.

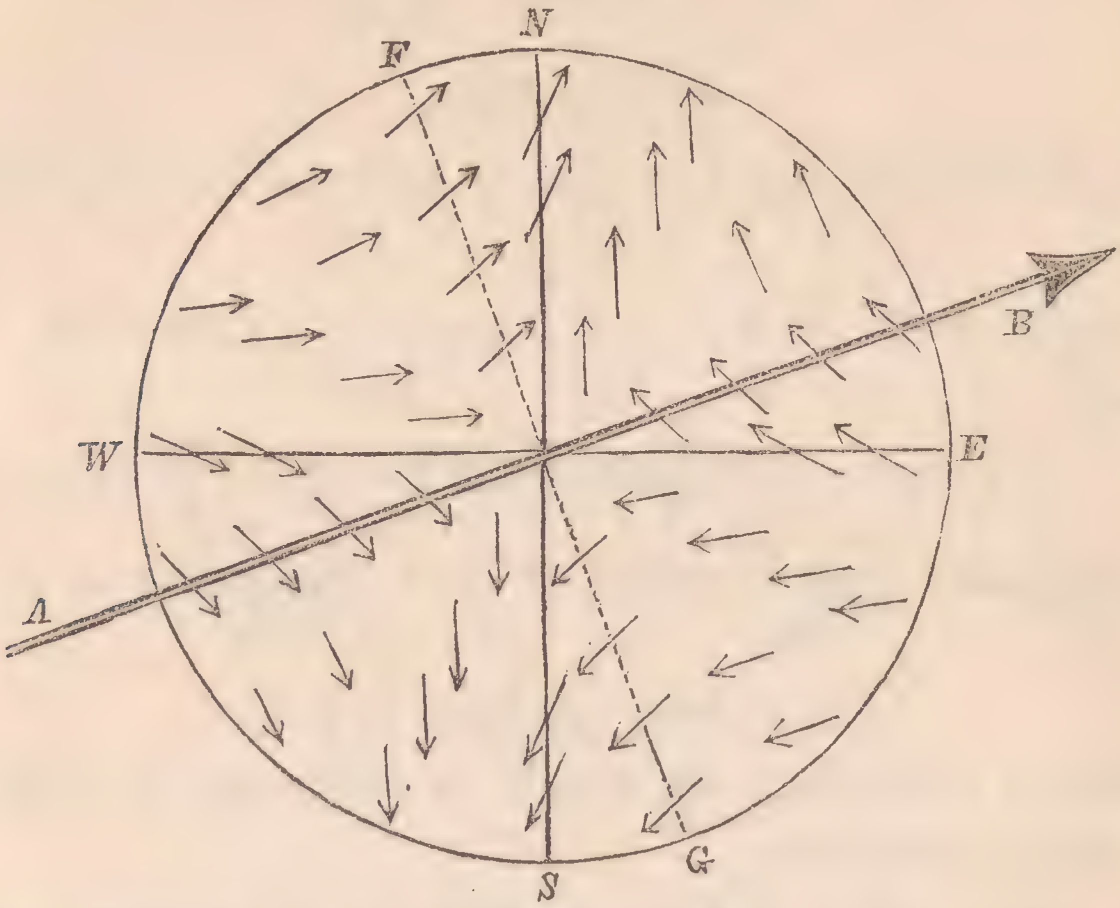


arrows in various parts, to represent these directions, we at once perceive that in the forward half, *F N E G*, they are from easterly, southeasterly, and southerly points, those along the axis, where the fall of the barometer is greatest, being from S. 45° E., the true point of minimum pressure ; while in the other half they are from northerly, northwesterly, and westerly points, those along the axis being from N. 45° W. ; thus harmonizing perfectly with the results shown in the wind-rose. If it be inquired whether, instead of such a system of spirals, some other system might not be devised, which would satisfy the results of observation just as well, we answer that there may be one other, and only one, and that so anomalous, and contrary to every mode in which experience shows fluids to move, that it is not to be entertained for a moment. It consists in just making the arrows on each side of the axis exchange places, without altering their direction, as in Fig. 3.

We are, therefore, shut up to the alternative, either that the uniform relation which we have shown to exist between the direction of the wind and the rise and fall of the barometer is the result of no general law in the circulation of the atmosphere, or that there not only *may*, but *must*, exist eddies in the general current, in which the air moves in the spiral manner we have described. Now, it is remarkable that

this is the very manner in which the best observations show it to move in the region of storms, during which it is known that our greatest

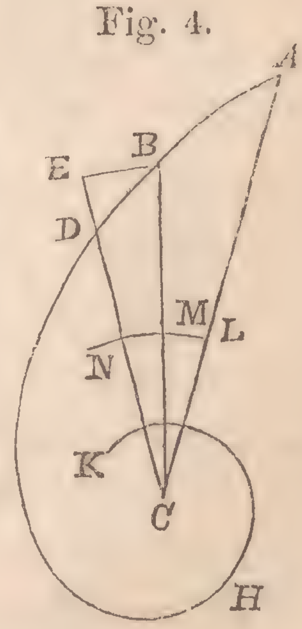
Fig. 3.



barometric changes are apt to occur; and our discussion seems to prove that the two great American champions on the laws of storms, with those who have followed the one or the other of them, on the other side of the water, are both right. The attention of one being chiefly directed to the evidence of rotary motion, he failed to make prominent the inward tendency, though I am aware that he has ever admitted the probability of its existence; while the other, laboring to establish the latter motion, omitted the former.

This spiral is easily constructed, and its properties discussed by means of its polar equation, which is exceedingly simple.

Fig. 4.



Let $A\ B\ H$ (Fig. 4) represent a portion of this spiral, C its centre, A and B any two points in the curve, and $B\ D$ an indefinitely small increment of the arc $A\ B$. With the centre, C, and a radius equal to unity, describe the arc $L\ M\ N$, and with the same centre describe the arc $B\ E$.

Put $A\ C = R'$, $B\ C = R$, the angle $D\ B\ C = p$, the arc $A\ B = z$, and the arc $L\ M = \omega$. Then will $D\ E = -dR$,* $B\ D = d\ z$, and $M\ N = d\ \omega$.

* Negative, because the radius vector diminishes as the arc increases.

By the similar sectors $C M N$ and $C B E$, we have the proportion.

$$C M : C B : : M N : B E.$$

That is,

$$1 : R : : d\omega : B E.$$

Hence,

$$B E = R\, d\, \omega$$

Again, in the right-angled triangle $B E D$, we have by, trigonometry,

$$B E = E D \,.\, \text{cotang.}\, E B D = E D \,.\, \text{tang.}\, D B C = -d R \,.\, \text{tang.}\, p.$$

Therefore,

$$R\, d\, \omega = -d\, R \,.\, \text{tang.}\, p, \text{ and } d\, \omega = -\frac{d\, R}{R}\, \text{tang.}\, p.$$

Integrating this latter equation, we have the equation of the curve, viz.:

$$\omega = -\,\text{Log.}^{*}\, R \,.\, \text{tang.}\, p + C.$$

To find the value of C, put $\omega = 0$, in which case R becomes R', and the equation becomes

$$0 = -\,\text{Log.}\; R' \,.\, \text{tang.}\, p + C.$$

Hence,

$$C = \text{Log.}\; R' \,.\, \text{tang.}\, p.$$

Substituting this value of C into the equation of the curve obtained above, dividing by tang. p, and recollecting that the difference of the logarithms of two numbers is equal to the logarithms of their quotient, we reduce it to the very simple form,

$$\frac{\omega}{\text{tang.}\, p} = \text{Log.}\, \frac{R'}{R}.$$

The following spirals were drawn from this equation, the length of the radius vector being computed at intervals of 45°, and sometimes of $22\frac{1}{2}$°. With the exception of the station at Bermuda, where the process was reversed, the computations commenced with a radius of five hundred miles, and were continued till it was reduced to ten miles; but the scale of the diagrams being too small to exhibit the inner curves distinctly, they were cut off at a distance of fifty miles from the centre.

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* Napierian Logarithm.

Fig. 5.
Ogdensburg.

Fig. 6.
Newfoundland.

Fig. 7.
Girard College, Phil.

Fig. 8.
Franklin Institute, Phil.

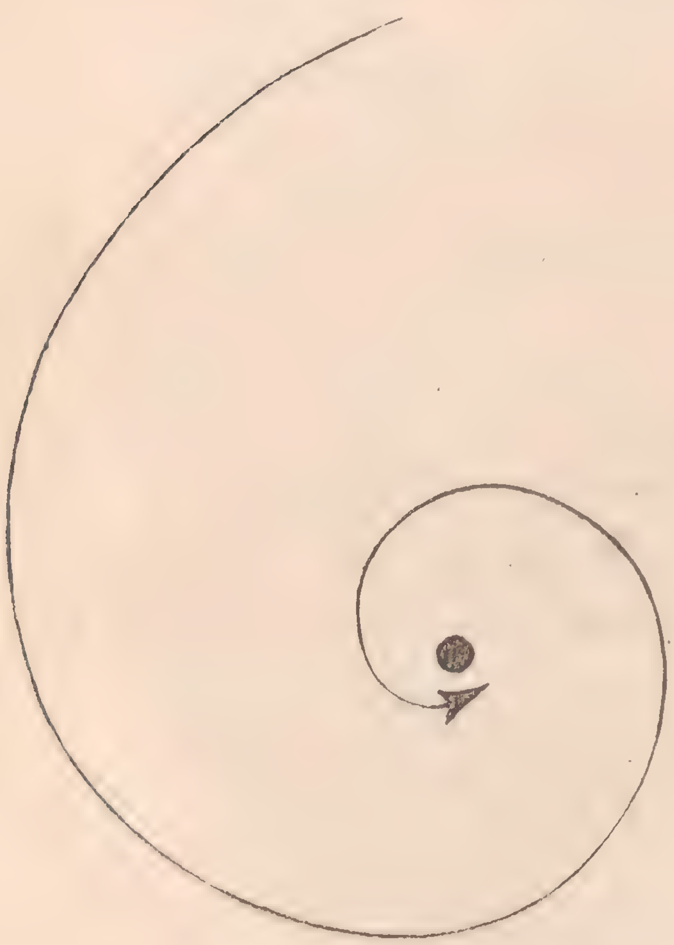


Fig. 9.
Boston.

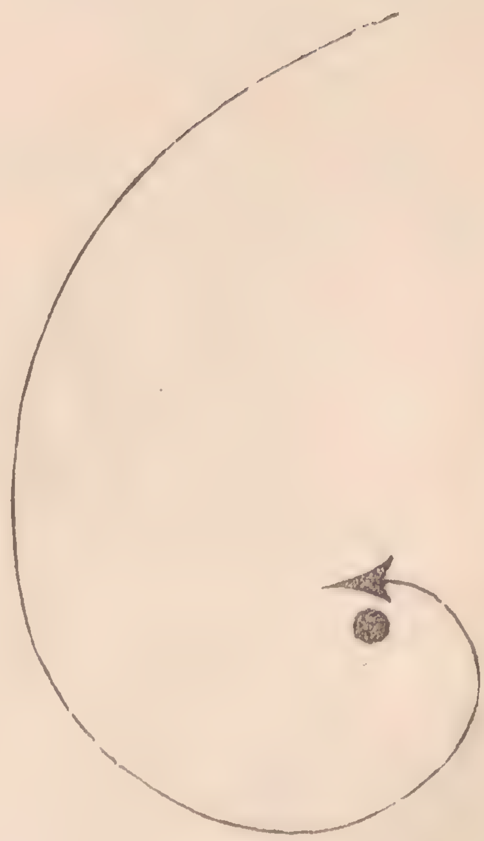


Fig. 10.
Nantucket.

Fig. 11.
Russian America.

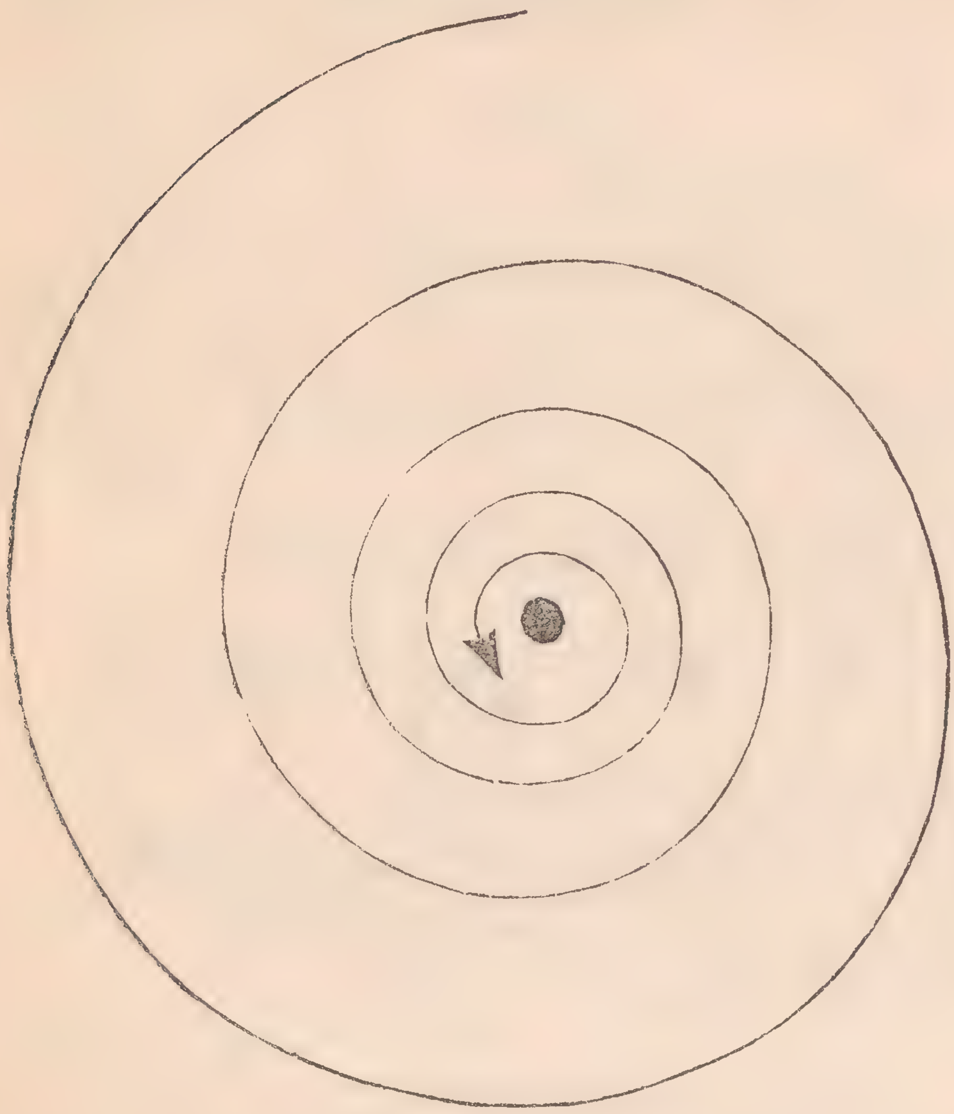


Fig. 12.
Iceland.

Fig. 13.
N. Atl. Ocean.

Fig. 14.
Bermuda.

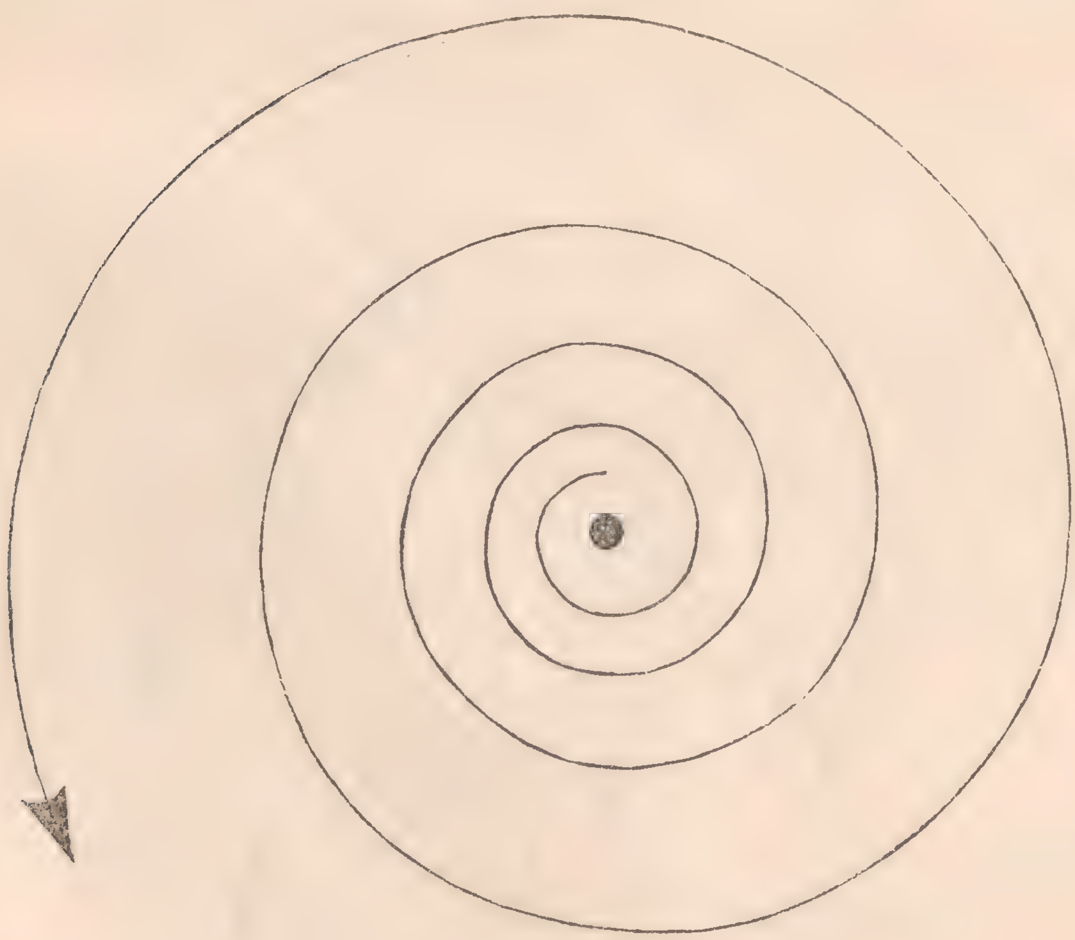


Fig. 15.
Greenwich, England.

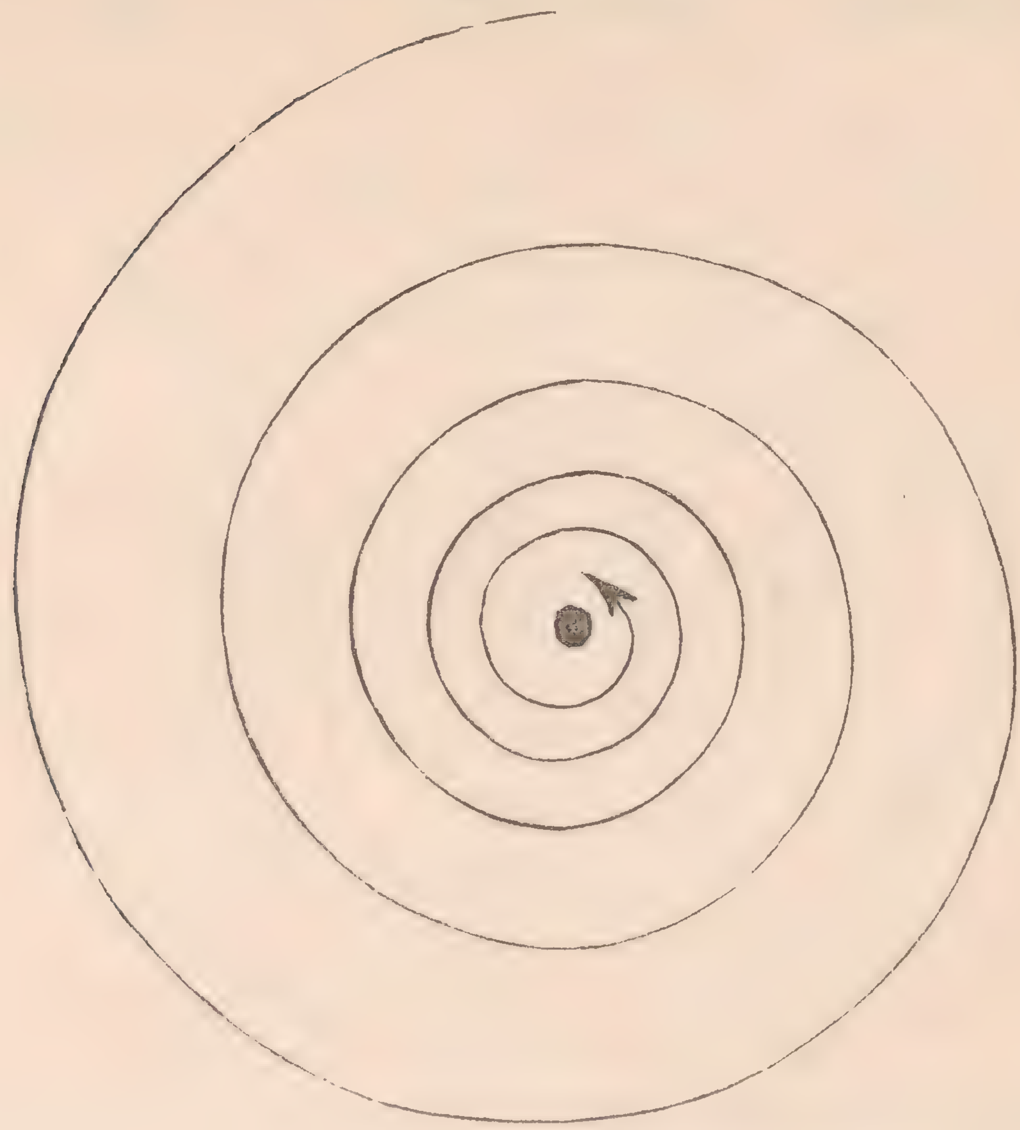


Fig. 16.
London, England.

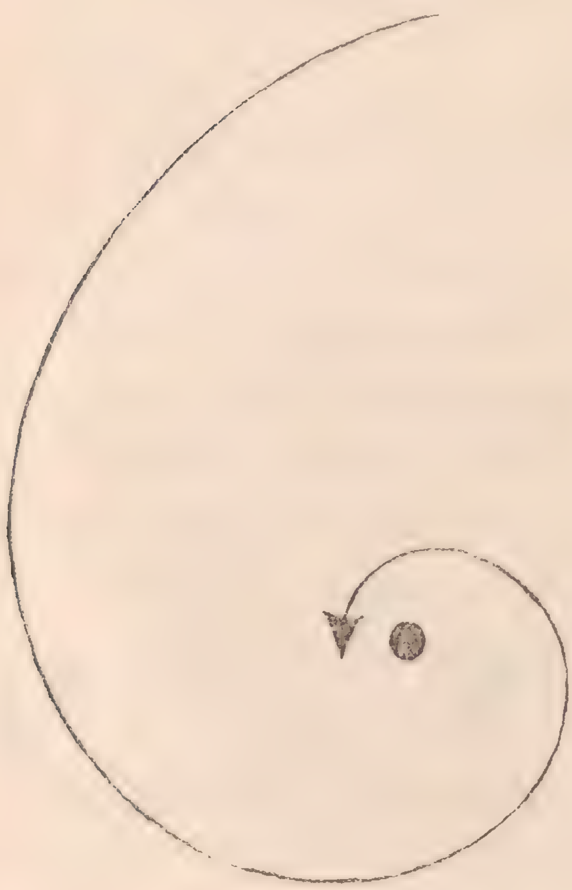


Fig. 17.
Paris, France.

Fig. 18.
Dantzic, Prussia.

Fig. 19.
Ural Mountains.

Fig. 20.
Barnoule, Siberia.

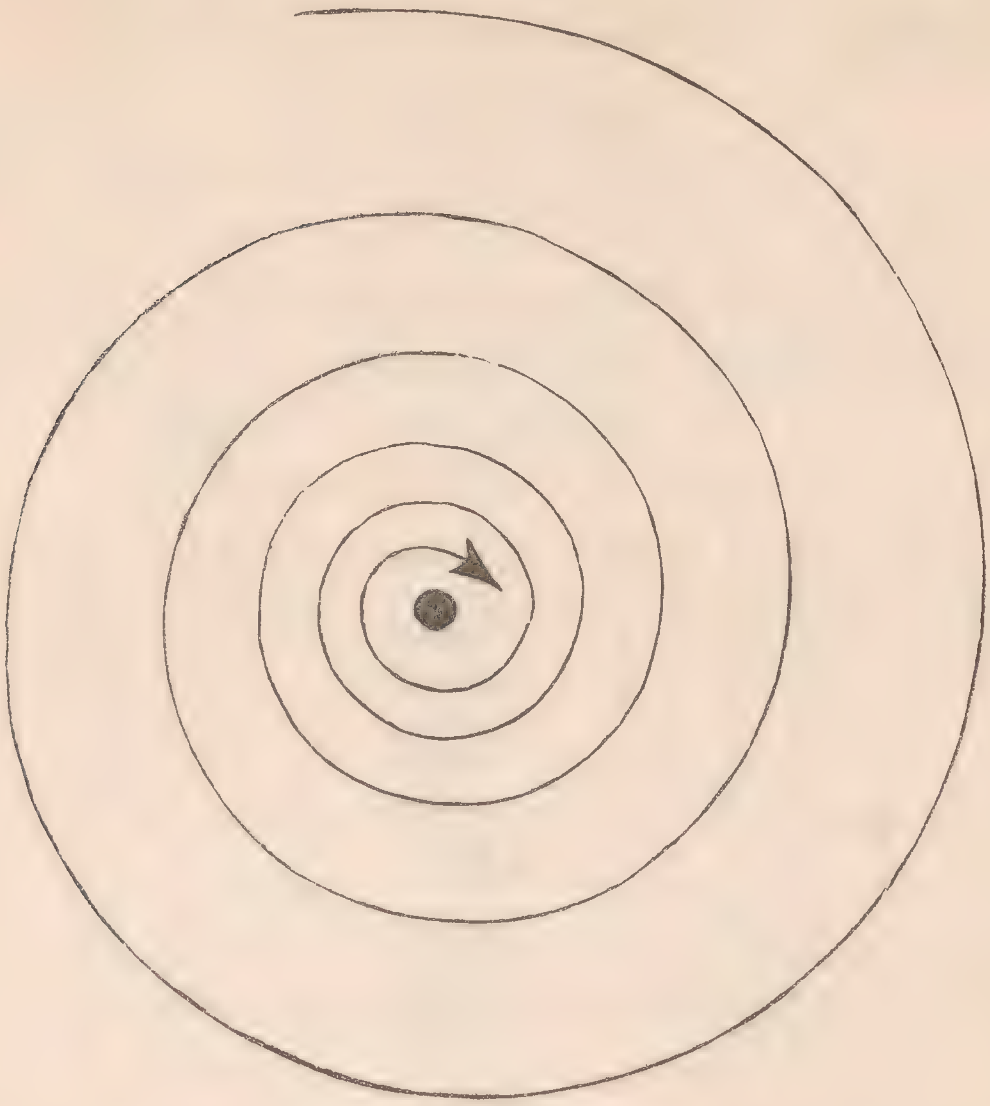
Fig. 21.
Pekin, China.

Fig. 22.
Average of North
American Stations.

Fig. 23.
Average of North
Atlantic Stations.

Fig. 24.
Average of European
and Asiatic Stations.

Fig. 25.
At Sea, in the Southern Hemisphere.



If any smile at the idea of delineating the track of a storm by means of a mathematical formula, I cannot help it. Nature will be more systematic sometimes in her operations than we are prone to imagine. It must be seen that in our process there is nothing fanciful, or left to conjecture. Grant our premises, and we cannot avoid our conclusion, even though we wished. And what are our premises? Why, simply, —

1st. That storms generally travel in the same direction as the mean atmospheric current; a fact that seems to be pretty well ascertained, at least in temperate latitudes.

2d. That the barometer sinks on the approach of a storm, and rises afterward; a truth well known to every observer.

3d. That the amount of the fall and subsequent rise diminishes as we recede from the axis, or central track, of the storm; and

4th. That the changes of the barometer at the time of storms are sufficient to control the general result; so that laws found to be true in the latter may be assumed to be so in the former.

These premises being granted, together with the relation that we have proved to exist between the direction of the wind and barometric

changes, the curves must necessarily take just the form and direction they do. We cannot make them curve more or less, or revolve in a different direction.

TABLE III.

Storm Curves.

Place of Observation.	Inclination of Curve to Radius Vector.	Reduction of the Radius Vector during 1 Revolution, expressed in Decimals of the Radius at the commencement of the Revolution.	Number of Revolutions and Degrees required to reduce the Radius Vector from 500 Miles to 10 Miles.	Length of Curve described in the preceding Column.
	° ′		Rev. °	Miles.
Ogdensburg	67 11	.92887	1 187	1,264
Newfoundland	62 25	.96246	1 69	1,058
Girard College	29 8	.99999	0 125	561
Franklin Institute	76 25	.78086	2 208	2,086
Boston	69 24	.90921	1 236	1,393
Nantucket	34 24	.99989	0 154	594
Russian America	85 18	.40342	7 206	5,980
Average	60 36	.97098	1 38	998
Iceland	41 24	.99935	0 191	653
North Atlantic	43 18	.99879	0 211	673
Bermuda	94 50	—.70110	—7 131	5,815
Average	59 51	.97400	1 26	976
Greenwich	85 41	.37765	8 89	6,510
London	73 0	.85356	2 13	1,676
Paris	59 30	.97528	1 21	965
Dantzic	58 44	.97795	1 9	944
Ural Mountains	51 3	.99374	0 277	779
Barnoule	35 16	.99986	0 158	600
Pekin	60 28	.97154	1 36	994
Average	60 32	.97126	1 37	996
S. Hemisphere	85 54	.36261	8 189	6,853

These curves admit of easy and exact rectification, and in the fourth column of the above table I have given the entire distance travelled along each curve while the radius vector changes from 500 miles to 10 miles. The second and third columns were computed from the equation of the curve, in which it is obvious that, if any three of the four quantities ω, p, R, and R' be given, the other may be found. The fourth column was computed thus. In the right-angled triangle $B\,E\,D$ (Fig. 4), $B\,D$ is equal, by trigonometry, to $E\,D$ multiplied by the cosecant of $E\,B\,D$, or by the secant of $D\,B\,C$. That is,

$$d\,z = -\,d\,R \text{ sec. } p.$$

The integral of this is

$$z = -\,R \text{ sec. } p + C.$$

To find the value of C, put $z = 0$, in which case R becomes R', and the equation becomes,

$$0 = -R \text{ sec. } p + C.$$

Hence, $$C = -R' \text{ sec. } p,$$

which value, substituted into the above integral, gives us the equation,

$$z = -R \text{ sec. } p + R' \text{ sec. } p = (R' - R) \text{ sec. } p.$$

That is, the length of the curve between any two radii vectores is equal to the difference of those radii, multiplied by the secant of the angle which the curve makes with them.

While the general features of these curves are the same throughout, there is considerable variety in their specific forms, depending on the value of the angle p, which ranges from 29° 8′ at Girard College, up to 94° 50′ at Bermuda. They may generally be rendered more uniform by employing the mean direction of the wind for a considerable extent of country around the place of observation, so as to neutralize local influences, to which almost every place is more or less subject, instead of that for the place of observation itself only. Thus, at Girard College, situated in the northwest part of Philadelphia, and having the rarefaction caused by all the city fires toward the southeast, the mean direction of the wind is unquestionably too far north, and so making the angle p too small. But if, instead, we employ the mean direction for Philadelphia, according to the observations reported by the Franklin Institute, taken, I presume, in a more southerly part of the city, or from Trenton, New Jersey, or New Castle, Delaware, some thirty or forty miles from it on either side, or the mean of the four, the angle p becomes much larger, and the form of the curve very nearly the same as the mean at the other American stations.

It is noticeable, further, that when the angle p differs but little from 90°, a slight change in its value materially affects the appearance of the curve, much more than when the angle is smaller. Thus, for example, a variation of less than 13° in the value of p converts the curve for Greenwich (Fig. 15) into the apparently very dissimilar one for London (Fig. 16); while the curves in Figs. 7 and 13 appear so nearly alike that the eye can hardly detect the difference, and yet the angle p is over 14° greater in the latter than in the former. Indeed, it is apparent from the equation itself, that the number of revolutions required to reduce the radius vector a given amount varies as the

tangent of p, which we know increases rapidly as the angle approaches 90°. If the angle were 58°, it would require but one revolution to reduce the radius vector from 500 miles to 10 miles ; if 78°, three ; if 88°, eighteen ; and if 89°, nearly thirty-six.

We have thus, as it seems to me, another link added to the chain of evidence going to establish the fact of the rotary motion of the air in storms, combined with an inward tendency ; and by a process so entirely unlike that of others, that it is, as it were, the testimony of an independent witness. Though in most of his published diagrams Mr. Redfield has represented the air as revolving in circles, yet in his earliest papers, and before the rival theory had any existence,* he alluded to the inward tendency, with a conjecture as to its amount. And considering that he gave it only as a conjecture, it is remarkable how near he came to truth, if our results are to be relied on. He remarks: "The degree of vorticular inclination in violent storms must be subject, locally, to great variations ; but it is not probable that, on an average of the different sides, it ever comes near to 45° from the tangent to the circle, and that such average inclination ever exceeds two points of the compass may well be doubted." By examining Table III, it will be seen that at three of the stations, namely, Nantucket, Girard College, and Barnoule, the inward inclination, according to the observations, considerably exceeded 45°, but the average of the whole is but 28° 9′, only 5° 39′ more than Mr. Redfield's estimate.

Mr. Piddington has given a diagram of the famous track of the Charles Heddle, which scudded round and round the centre of a storm in the Indian Ocean for five successive days, and from the most accurate measurements that I can make, it appears to have been drawn inward about one third of the radius during each revolution. This would make the inclination not over four degrees, and the curve to very nearly resemble ours for Greenwich.

I infer that Professor Loomis estimates the inward tendency considerably higher; for, in summing up the results in the storms investigated by him, he says: "In all we find certain common characteristics, namely, an inward motion, with a tendency to circulate against the

* Unless we regard the conclusion arrived at by Dove, in regard to an isolated storm on Christmas eve, 1821, as originating the theory, which, I believe, he did not claim. (See Piddington's Horn-Book of Storms, page 3.)

sun"; thus making the inward motion the most prominent of the two.

The results of the widely extended system of investigation now carried on under the direction of the Smithsonian Institution, in conjunction with others, will be looked for with much interest; and if the beautiful plan devised by Professor Loomis shall be adopted for exhibiting them, I hazard the prediction that the mean will be seen to differ not far from mine. Not that mine will be exactly realized in any individual storm, but that it will be found to be a mean toward which all storms approximate more or less closely; or, rather, I ought to say, the system of investigation that I have adopted will, if carried out, give us such a mean.

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5. On the Distribution of Precipitation in Rain and Snow on the North American Continent. By Lorin Blodget, of Washington.

The Smithsonian Institution has, through the last four years, directed a widely extended system of meteorological observations, in which measurements of the atmospheric precipitation in rain and snow had a prominent place. The number of stations reporting this most important observation began at about fifty in 1849, increasing to about one hundred and fifty in 1852. The New York University system has also directed and continued the observation of the fall of rain and snow at the Academies in that State, from near the commencement of its meteorological observation in 1825, and the now very extensive system of observation at the military posts commenced observations of the fall of rain in 1836. Private individuals, also, and separate institutions, have added largely in separate series, of variable periods. From all these sources, and from the observations placed at the disposal of the Smithsonian Institution, and, for the time since 1849, made mainly under its direction, the number of reported measurements available for combination and generalization was one hundred and fifty in 1849, increasing through each year, to two hundred and fifty in 1852.

Upon this collection of returns for each year separately, the attempt

was first made to deduce results. The sums at the several stations for a certain month were grouped geographically, or in a climatology, so far as could safely be predetermined, in such a manner as to bring the several returns into a position of criticism and comparison with each other. The scattered sources and diverse forms of measurement at first seemed to render this impossible, and to prevent anything like comparable presentation; but the number was so great that symmetry of arrangement in results soon became apparent from this symmetrical arrangement of observations.

Tables of results of observation were prepared in this manner, giving the sums of precipitation in every form for each month, season, and year, for the four years referred to; and upon these tables, charts of graphic illustration of the observed conditions were drawn for each of these separate periods, seventeen for each year, and sixty-eight for the whole period.

Subsequently, all who were known to have made observations of the fall of rain for any period were solicited to place their observations, or a summary of the results, at the service of a more general combination and comparison; and, in response to these requests, many series, embracing long periods of observation, in detailed tables, or in summaries, have been transmitted to the Smithsonian Institution. A general summary of results was then attempted upon these materials, including the New York Regent's Reports, from the commencement to the present time, and the entire amount of observation at military posts, also to the present time, the last ten years of which are in manuscript.

In this general summary of precipitation on the North American Continent, the arrangement peculiar to the first-named illustration of single months and years was preserved, bringing all stations in their proper relation to form a criticism upon each other, and giving the mean results for each month, season, and the year. Four hundred stations were found to give sufficiently accurate and reliable results to warrant their incorporation in the tables, and for this number, means were obtained for variable periods, from one to sixty years. The whole number of years observed is two thousand eight hundred, made up by adding the number of years of each separate period, and giving a mean, therefore, of more than five years of observation at each station. The immense amount of unpublished observations now collected

and concentrated by the Smithsonian Institution can alone verify the existence of such extensive data on this subject, and its extraordinary facilities and effective direction only could render these available for the great practical and scientific purposes to which they may be applied.*

The most striking result of the discussion of the observations of the fall of rain on this portion of the continent is the development of a *system of symmetrical distribution.* This most important condition may therefore be investigated in a much more effective manner than if the reverse were the case, as on the Eastern Continent, and generally elsewhere. We have a condition here very nearly reversing that of Europe, indeed, in respect to the immediate inducing causes of precipitation, and these inducing causes are necessarily associated with distribution.

Precipitation by contact of saturated volumes of air with portions of the earth's surface has usually been considered the great inducing or initiatory process, and excessive precipitation was found on hills and mountains, therefore, which offered abrupt contact with the saturated atmosphere from adjacent seas. To generalize upon precipitation so induced or initiated, and from such sources of supply, would scarcely be possible, farther than to refer it to the superficial atmosphere, and to these obvious causes. Such has been, to a great extent, the theory of precipitation and of supply of moisture to continents held hitherto.

Before going on to show the essential difference between these conditions and the facts actually existing here, and the inadequacy of this hypothesis for their solution, we may refer to some facts which verify it for portions of the Eastern Continent, and especially for England. An immense amount of observation has been made in England upon the fall of rain and the general precipitation in every form, yet few general or comprehensive deductions have been drawn, beyond the mere determination of its eminently local character. Each district seems to stand by itself, and no one of these districts to illustrate another in any degree.

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* The tabular matter and illustrative charts presented with this paper are not introduced here, as they belong more appropriately to the full report of reductions of meteorological observations, prosecuted under the auspices of the Smithsonian Institution. The matter is exceedingly voluminous, and detached portions would be only illustrative. The reader is referred to that report for the full amount of matter and complete illustrations.

As a rule, however, so far as a rule of distribution may be considered as attained, the highest elevations have the greatest amount. As all the elevations observed are of little absolute height, this verifies the correctness of the hypothesis referring the supply to the lower volumes of the atmosphere, and determines the propriety of looking no farther than the surface winds for its means of transit from the evaporating to the precipitating surface.

That this, however, is a cause sufficient to produce the entire class of phenomena of precipitation in Europe, or even in England, I cannot believe; but it masks the effect of more general causes so completely, that little if any attention has been given them. Districts differing but moderately in elevation have great differences in the amount of rain in all parts of England. Farther north, greater and more excessive amounts fall upon the heights which break the western ocean winds. Eighty inches, or even a hundred, fall in some localities; and even in Central England it is usual to find on the low mountains, or hills, as they are designated, an amount twice as great as that falling on the adjacent plains.

More abrupt contrasts still exist in Southern Europe, — the region of the Pyrenees, and even in the Alps; and so predominating is this extremely irregular and diverse distribution, that the precipitation of the great area of Northeastern Europe seems to have received little attention, and it may, therefore, and probably does in reality, present other conditions. There are many reasons for believing that portion of the continent to have features in common with the eastern portion of this, in the character of its precipitation and in its sources of supply, but we are as yet without data to determine the point.

The first result of the grouping and comparison of the observations of precipitation on this continent was the discovery of a general symmetry and correspondence in the amounts reported at stations near each other, or in the same general physical relations of elevation, distance from the sea, and latitude. So complete has this symmetry proved to be, that graphic illustration of any single month's amount gave concentric areas of lighter shading from a maximum district outward, and a generally beautiful proportioning of amount, without regard to peculiarities of surface. In these charts of monthly distribution, the constants were scarcely apparent, and the maximum districts fell alternately in every part.

Next, *the absence of any increase in amount due to elevation* became a most conspicuous determination. So far from the Alleghanies adding to the amount normally falling in the latitudes they occupy, they were found to give *markedly less* than the Ohio and Mississippi Valley, and less than the Atlantic coast, on any line of latitude where corresponding observations could be obtained. And this diminution in quantity seems to be fully as great as that due to the decreasing temperature of higher latitudes; as a point at an elevation of three thousand feet, giving approximately a decrease of temperature of ten degrees in the mean of any month, would have no more precipitation than a point far enough north to give the same mean temperature. The Board of Commissioners of Public Works in Virginia, unwilling to accept the reported small supply to feed the navigable rivers of that State, placed gauges at the opposite bases of the principal range of the Alleghanies, and the mean annual quantity for four years is but thirty-six inches, while for the same period the Ohio Valley has forty-seven inches, and the plain of the Atlantic coast forty-three, in the same latitude.

Equally decided is the elimination of local effect in every portion of the United States east of the Rocky Mountains, except, perhaps, a portion of the lake district in New York, and some elevations in Massachusetts, and the important distinguishing fact may be regarded as established, that the Alleghanies, as a whole, greatly diminish the amount of precipitation in the districts they occupy, though the results actually appearing here exhibit fully the same symmetry, and the same connection of areas of various amount across their line, which prevail elsewhere.

The *mean annual distribution* preserves this symmetry and correspondence of adjacent stations and districts, and defines it more clearly than the distribution for separate months may be defined. It also discloses new laws of constant distribution, and points more clearly back to the primary sources of supply of moisture to the continents. In this case, and for the practical purposes of the illustration, the apparent general laws of mean annual distribution may be stated as having two rules of graduation; one of which is a *regular decrease of amount with decreasing temperature*, and the other, or concurrent rule, a *decrease of amount with increase of elevation.*

In accordance with the first, the maximum of this portion of the

continent east of the Rocky Mountains is found near the Gulf coast, and a regular decrease appears along a right line across the thermal lines northward. The principal exception is on the plains and west of Missouri, and partial exceptions are found in the dry plain of Wisconsin, and the Ontario Valley in New York. The first of these may not be expected to accord entirely, even if its general law of distribution were as comprehensive as that which holds for the continent eastward, for the reason that the configuration and extreme climates of the Pacific coast and the Rocky Mountains must, from their position, greatly modify the climate of the whole area of the plains.

The other districts alluded to are less important anomalies, though sufficiently marked and decided in their character as much the driest portions of the continent, for their temperature, east of the plains.

The general position, however, remains, and in accordance with it we find an annual mean of near *sixty* inches of water falling at Natchez, and in Central Alabama and Georgia *fifty-five inches*, the area of excess extending up the Mississippi River and toward the Atlantic coast, and an amount of *fifty inches* reaching to Camden, S. C., and nearly to Cincinnati on the Ohio, and to Muscatine, Iowa, on the Mississippi. In this last direction the mean of later years adds much to that first measured at St. Louis, and the correspondence of recent years of observation at different places is such, that we cannot doubt the accuracy of the measurements.

Concentric areas of decreasing amount thrust themselves northward from these maximum districts, mainly in accordance with the apparent rules.

There is one general exception, however, which should have been noticed earlier. It is a *decidedly less quantity on the immediate coasts*, especially in the south. This may be fully accounted for in the coast wind and cooler atmosphere of those localities during the portions of the day most liable to profuse precipitation. We have, then, a remarkable reversal of previous generalizations on continental precipitation, which gives us, in the present case, a *maximum line in the lowest and farthest interior* of a great continental area, — the Mississippi Valley giving fifty inches where the Atlantic coast gives forty-two, and the Ohio Valley giving forty-eight in latitudes where the Alleghanies give but thirty-six and the Atlantic coast but forty-two.

This interior distribution is, in part, a law overruling the local

effects so conspicuous in Europe and on the Pacific coasts, and is in some degree itself anomalous. The quantity falling on the Upper Mississippi and Ohio is too great for the rigid application of the rules here given. Iowa seems, indeed, to be a focus of excessive precipitation, in striking contrast with the plains westward and with the lake country. It is intensely heated, however, and very low for a continental area so far in the interior, and it may be that a more extended series of years will remove a share of the excess in amount that seems now to belong to it.

These general facts of the distribution of rain on this continent point, by the clearest induction, to a *source of supply entirely disconnected from the surface winds and surface atmosphere.* We have no gradation in amount from oceanic districts towards the interior, but a law of distribution on this portion of the continent apparently disconnected from any oceanic considerations. Local areas of evaporating surface, also, if they influence precipitation at all, seem to diminish rather than to *increase* the amount. Such is certainly the case about the great lakes.

The ocean winds which reach this portion of the North American continent are few, and of little consequence in the amount of air they transfer. Professor Coffin has shown the resultant movement of the surface atmosphere to be uniformly and markedly from the land toward the sea on the whole Atlantic Coast to 35° north latitude. On the Southern and Gulf coast, the resultant for the year is also from the coast, computing the force and number of winds observed, and interpreting them into the actual volumes of air transferred. The violent and spasmodic winds from the land overbalance the longer continued and moderate aspiration from the sea, and the trade wind becomes initiated in this belt of violent alternations, by the reduction of its movements to their true resultant, before appearing in its regular and recognized development.

The southeast winds attending or preceding storms in the interior are rarely or never continuous from the sea in that direction. They are as decided at Pembina, Minnesota, in latitude 49° north, as at New York, or Camden, South Carolina, and in neither case bring saturated volumes from the sea to create a storm in the interior. This character of interior origination, and the precession of the surface winds backwards, or the transfer of the area occupied by them toward

the point from which they blow, has long been understood in the case of northeast winds and storms. Nearly all storm winds form a class quite analogous in every important point; and, though they may add in some degree to the amount of precipitation in general, and to that of severe storms particularly, they are never the inducing causes, and never give, from the volumes of ocean air they bring to this portion of the continent, any considerable amount of precipitation.

The considerations necessarily connected with the phenomena of distribution illustrate these positions. A storm commencing in the Mississippi Valley, and attended by both southeast and northeast winds, *while west winds are blowing on the Atlantic coast*, may distribute about Wheeling, as its maximum point, four inches in depth of rain, with none falling at a distance of four hundred miles in every direction. It must be obvious that this immense amount has some great and general source of supply, in which the lower atmosphere and surface winds hold an entirely subordinate place. The distribution for each month, season, and year presents the same comprehensive character, and similar instances of concentration and gradation alternately falling on every district of the interior, and it refers necessarily to the same general and remote supply.

For this supply we can see no cause adequate except the returning equatorial current, bearing with it an amount of moisture which it cannot sustain in the colder latitudes, and under the condensation to which this loss of heat, and consequent descent of its volumes, reduces it.

Descending, as these volumes do, and changing from southwest to west over the belt of descent, the supply is uniform, and *instituted at every point*, though the conditions which may influence the time and manner of its precipitation may be quite irregular, and the solution of the problems respecting the initiating processes and the attendant forces extremely difficult.

6. On the Southeast Monsoon of Texas, the Northers of Texas and the Gulf of Mexico, and the Abnormal Atmospheric Movements of the North American Continent generally. By Lorin Blodget, of Washington.

The portion of the North American Continent lying west of the Mississippi River has been, until quite recently, almost unknown in regard to even the more striking and obvious of its conditions of climate. We have known, vaguely, that it had extraordinary conditions in some respects, and such as differed widely from those characteristic of the same latitudes on the eastern portion; but the definition of these abnormal conditions, if they were such, and their reference to the proper causes, has not been made. We have had too few facts, indeed, to warrant any generalization until now; when, by the establishment of military posts, and by the aid of officers in charge of special surveys, and of individual observers, the accumulated observations of two or three years become sufficient to throw great light on the more striking phenomena of these Pacific and interior climates.

We have here, in this portion of the continent, so little that is not common to the temperate latitudes of Western Europe, in winds and surface movements, that unusual or continuous winds strike us as most extraordinary, and are, therefore, best observed.

The whole Atlantic and Southern or Gulf coast presents us with more or less marked alternations of land and sea breeze, with all the more common phenomena of which, and their solution, we are quite familiar. But toward the western extremity of the Gulf coast, in Texas, this becomes magnified quite beyond the ordinary phenomenon of land and sea breeze, and develops a *true monsoon*, or continuous wind, day and night, for months together. At the mouth of the Rio Grande, and between this point and Corpus Christi, the maximum effect is produced, and the fully developed monsoon reaches to the interior desert plain, called the *Llano Estacado*, or *Staked Plain*, and blows over the whole district bounded on the southwest by the Rio Grande for the portion nearest the Gulf of Mexico, and by the San Pedro and Pecos for the interior portion, and by the country between the Nueces and Colorado of Texas on the east, from May to November.

On the western coast of Mexico, in somewhat lower latitudes, the

trade wind of the Pacific is quite neutralized by causes analogous to those producing the first phenomenon, and the effect here extends to some distance at sea. The peculiarities of the cause here will be more particularly alluded to farther on. For some portions of the summer months the trade wind of this latitude is quite reversed, and a southwest wind is substituted for the normal northeast trade. Lieutenant Maury has fully substantiated this fact of the reversal of the trade wind, and he considers it at times equal to the normal easterly winds of this latitude in velocity of movement. Observation of this coast is, however, quite irregular, and, except at Mazatlan and San Blas, such only as the transient passage of vessels could make.

On the California coast, a continuous wind is experienced, of so marked a character as to attract great attention. The northwest, or north-northwest wind, unfailingly occurs here in every summer day, and on the immediate coast it scarcely ceases at all through the summer, continuing through the evening and night as a slight landward aspiration. As this blows, however, without being aided by the circulation of surface atmosphere usually received as the normal one, it has not the absolutely continuous character it would have if it accorded, or nearly accorded, with that circulation. The monsoon of Texas does so nearly accord with this circulation, expressed there by the northeast trade as it enters the Gulf, deflected by local circumstances to an exaggerated sea-breeze on all the Gulf coasts, that it more readily blends with that movement, and deflects it merely to a true southeast continuous wind, which is partly trade and partly monsoon.

In the lower valley of the Rio Grande, and one hundred miles from the coast, the meteorological observations at Ringgold Barracks give a resultant atmospheric movement, in April, from S. 65° E., of 1.75 in the scale of estimated force of winds in use in the Military Registers. In May, the resultant is, from S. 19° E., of 3.3 in force; in June, S. 26° E., of 3 in force; in July, S. 10° E., of 2.64 in force; in August, S. 5½° E., of 3 in force; and in September, S. 46½° E., of 1.5 in numerical estimate of force.

The resultants are from the land, and at northerly points in winter, but the preponderance is so great from the sea that the *annual resultant* is from S. 30° E., with a mean of 1.34 in the estimated nota-

tion of force. The precise interpretation of this estimate in miles per hour is quite difficult, but the number of 3, which is the mean of the summer months, should, by the best data of registered movement, as compared with the estimates which we possess, be placed at 12.5 miles per hour. We have, then, at a station considerably inland, and with a local deflection to the south derived from its position out of the direct track of the principal movement and southward of it, a continuous atmospheric movement, which cannot be ranked less than a full monsoon. At Fort Brown, very near the coast, the wind is southeast, without change through these months; at the other interior forts of Western and Lower Texas, Laredo, and Forts Duncan, Inge, Lincoln, Merrill, and San Antonio, the same resultants are deduced, with a few local modifications; but at Santa Fé, in the direct line of their extension, they do not appear, nor in any part of the upper valley of the Rio Grande, or beyond the mountains of the Pecos. Beyond Fort Atkinson, on the Upper Arkansas, and Fort Arbuckle, on the Canadian, and at all the posts northward and eastward, they are unknown.

It is perhaps scarcely necessary to say more in verification of the existence of the phenomena referred to here, except that they have been deduced, and their boundaries defined, by a correct reduction of the observations. The mean of these observations can scarcely be in error in the facts, though it may be in the degree of developments, from error in the scale of notation. A combination of the results, and a statement of the analogies which seem warranted by them, will assist in verifying the phenomena, as well as in solving the problems they present.

We will here refer to some comparisons of these continental anomalies of atmospheric movement with those of the Eastern Continent, and to their more striking points of similarity.

In the July number of the *Edinburgh New Philosophical Journal*, Professor Dove, of Berlin, treats, though somewhat incidentally, of the analogous atmospheric movements of the Eastern Continent, as exhibited in the Indian monsoons, the east winds of the eastern coasts of Asia, and the north winds from the Polar Seas. Referring them to one great and general cause, that of great rarefaction in the dry and heated area of the interior, he says: "A greatly diminished atmospheric pressure taking place over the whole continent of Asia must produce an influx of air from all surrounding parts." "The

monsoon itself becomes, as we see, in this point of view, only a secondary or subordinate phenomenon." Professor Dove goes on to show, by comparison of barometric means for the different portions of the year, the inducing cause of this general continental rarefaction in summer. The elastic force of vapor, which increases with the temperature when the supply for absorption is kept up, does not, from the absence of bodies of water from which it may be formed, keep pace here with the increase of temperature and the consequent rarefaction of the volumes of dry air. Continental areas of the interior are, therefore, nearly always points toward which winds of aspiration tend, though this inward draft may not be sufficient to develop a true monsoon. Europe has a local supply of moisture in the adjacent ocean sufficient to neutralize any rarefying effect its own surface may have, and that these are its real conditions is clearly shown in the *increasing* atmospheric pressure from the commencement of the summer months which occurs in Western Europe, instead of a progressive *decrease* in pressure through those months, as here and in Asia. The normal atmospheric movement in Europe from the west is probably accelerated by this continental rarefaction, and the same is probably true of the Pacific Coast of this continent, north of 40° north latitude.

These views not only illustrate the abnormal conditions of atmospheric movement, and enable us properly to separate and classify all these, but they also throw great light on the general system of atmospheric circulation. Separating and identifying the abnormal movements of the surface atmosphere of our own continent, we may then easily resolve the general and normal movements. Indeed, we may eliminate interior rarefaction alone, and its proper consequents, and take what remains as the truly normal condition of circulation, due to a purely cosmical distribution of heat, and to that alone.

The questions connected with the *overflow*, if such takes place, from the points of maximum rarefaction, are highly important in considering continental climates, and also in considering general and normal systems of circulation. Dove supposes that the ascending volumes of Central Asia may overflow toward Western Europe, and to the Polar Seas; and that the higher barometer there during the summer months may be due to this accession of atmospheric volumes. The irregular annual curve of barometric pressure may be directly due to this various rarefaction, or to the different measures of rare-

faction resulting from the contrasts in humidity between the continental interior and the parts adjacent to the sea, without supposing the first effect to be produced. A retardation of the course of a normal movement of the atmosphere, as from west to east in Europe, might be quite adequate to produce increased pressure in Europe, and this is much more likely to be the true solution of such a phenomenon than to suppose the swelled atmospheric volumes of Asia to have rolled over upon the west or north. Lateral pressure, from suddenly increased expansion, must be quite as effective as a heaped volume; and so mobile a fluid, if elevated in mass, would be held up like a floating body on water by the heavier portions adjacent, and by the gradual blending of the separate conditions.

The principal problem of these abnormal atmospheric movements is their relation to primary systems, or whether they are not indeed themselves primary, and therefore whether they are truly abnormal. Dove inclines to the opinion that they are, in a great degree, primary; and that they, in conjunction with the heat of the equatorial regions, originate the general atmospheric circulations. This point is of the highest importance in every climatology, and especially in that of this continent; and without concurring in the full extent of this view, we may be warranted in admitting an increased effect resulting from the conjunction of the two. Our precipitation on the eastern portion of this continent, for instance, may be greater for the intrusion of ascending volumes in Upper Texas into the regular current above, and the normal distribution of precipitation may thus be increased over a large district, without the supposition of anything more than a secondary and cumulative agency in this ascent of rarefied volumes.

Atmospheric circulation lies really at the base of all the atmospheric conditions we may observe on any single portion of the temperate latitudes. Dove alludes to the essential change in his views of the relations of these conditions in Europe, which has been induced by comparison of observations in other parts of the world, and especially in Asia. Our own conditions, both constant, and disturbed, or dynamic, are pre-eminently referable to remote and general causes, and to *systems* of atmospheric movement.

The "*Norther*" of Texas and the Gulf coast, as not coming in the same class of winds of continental aspiration, has not been referred to in the foregoing. The statistics of this phenomenon are, it should be

confessed, scarcely sufficient to solve the questions connected with it, or even to show fairly in any exhibit of it. But some interesting facts have been collected and some analogies seem so clear that it may be proper to present them.

Observations of the Northers, so far as to identify them, are sufficiently numerous on both borders of their extension, north and south, and their limit west is even clearer, so far as identity with the Gulf is concerned. The San Pedro River of Western Texas, and the mountains bordering the Pecos, form the line of distinct boundary, not only of this phenomenon, but of the general conditions of climate in that part of Texas. The irregular and violent winds of New Mexico are not analogous to the "Norther," though often from the north. Their characteristics are those of the most decided irregularity, and the most purely local of all winds. On the north, Fort Atkinson, at the crossing of the Arkansas, is also beyond them, and indeed they seem limited nearly to the district of the southeast monsoon, though reaching, in a modified character, farther east and farther west. At the south, their limit is *within* the waters of the Gulf, prevailing, as I have been assured, sometimes within sight at the northward, while the expiring trade wind from the east continued its moderate but constant course at the point of observation.

Having thus bounded the phenomenon in every important direction, we may present some of its peculiar characteristics. Dr. Berlandier, a French physician of great ability and accuracy of observation, made meteorological observations for many years in Northern Mexico, at San Luis Potosi, and other interior towns, but mainly at Matamoros. Furnished originally with instruments by a society of physicists at Geneva, and reporting his observations there, he afterward continued a long series for his private purposes. His death occurred a year or two since, and the manuscripts were purchased recently by Lieutenant Couch, of the U. S. Army, and are fortunately preserved. I am permitted to quote the following notes of Berlandier, descriptive of the norther, and of some of the conditions attending and preceding it.

"*Sound of Breakers, pretended Heat of Waters of Lakes before Changes of the Weather, &c.*

"The study of meteorology has in some degree preceded that of *physique;* and as in all countries, as in all weathers, the inhabitants

observe attentively the natural phenomena for the purpose of predicting changes in the atmosphere, I consider the notions respecting changes and the maxims of the inhabitants of these coasts and the people of the country in general entitled to much confidence.

"Among the most important signs of change of weather here, I cite *the sound of the breakers of the coast*, a very distinct sound, many times scarcely to be perceived on the coast, and at others extending ten to fifteen leagues into the interior. Inhabitants of the coast have observed that, when the wind is about to change, the sound of the breakers changes its direction, and is heard in the direction from which the wind is about to come. After having been heard for a time of greater or less duration, the sound diminishes gradually, and at length ceases entirely, as the new wind appears on the surface of the earth. This phenomenon is remarked particularly in winter, when the wind blowing at first is from the south, and the wind from the north is about to come on.

"These phenomena, confirmed by long observation, throw great light on *the point of origin of winds*, and confirm the observations of Franklin, who was the first to remark that violent north winds commenced to blow at the south, and that it is not until after many hours that they arrive at the northern regions.

"The 1st of May, 1847, an Italian sailor was stationed on the coast of the Gulf in correspondence with me at the rancho of Mezquital, and we fully verified the phenomenon. Watching the sound of the breakers, it came at length to be distinctly in the north, but that which fixed our attention still more was an extraordinary heat of the waters of the Laguna Madre, and the presence of a series of cumulous clouds, at an elevation of 60° above the northern horizon. This appearance remained through the morning. Finally, an hour after midnight, the wind commenced from north-northeast, passing to the north-northwest in the lower regions of the atmosphere, at the same time blowing from the west in the higher regions, and as the wind began to blow, the noise of the breakers ceased.

"We assured ourselves by positive observations of the extraordinary variation of temperature, and most unseasonable heat of waters and trees, and I do not believe it to be produced by the intense heat of the previous days; to what other cause it may be due, I cannot tell. It may serve the purpose of other meteorological observers attending to

this phenomenon, to know that it is not only remarked of the lakes near the coasts, but that from the reports of people of the country I am assured that the waters of wells not far from the coast (fifteen or twenty leagues) exhibit also the same changes of temperature when a change of weather is about to occur.

"The low barometer was very notable on the 1st of May, the day referred to, and the heat of the atmosphere excessive."

In a note to the register for December, 1843, Berlandier remarks on the low elevation of the mass of cloud in the norther. Speaking of its appearance in approaching, he says: —

"On the 31st of December, 1843, the south wind had blown all day, and continued moderately through the evening. I had prepared to take the altitude of several stars, when I was struck with the appearance of the horizon in the north-northwest, — a species of circular band appearing there, with its convexity toward south-southeast. Thinking it, at the first moment, an aurora borealis, I observed its phases with attention, and soon saw it to be an immense cloud. A remote sound of a storm was heard, the wind at south changed to west when the front of the cloud attained an elevation of 15°, and then the north-northwest wind began, continuing through the night. In less than one minute this frightful cloud had covered half the sky, in strong contrast with the perfect serenity of the other portion, in which the moon was shining brilliantly. Taking the height of the cloud at 45°, there appeared a yellow-white sphere, greater than our planet, traversing the higher regions of the atmosphere. Continuing its course, the northern region exhibited a clear spot as the meridian was obscured. In the south-southeast the lower border was also circular, white, and a slice from the opposite border appeared above the northern horizon. At $8^{h.}$ $50^{m.}$ all had disappeared, and the sky had but slight traces of any cloud remaining.

"The perfectly homogeneous character of the cloud was the most conspicuous feature of grandeur. Arrived at the zenith, it appeared to envelop the earth, covering just a half-sphere, east, west, and north. The color was a yellowish-white, analogous to that of the moon in the telescope; it appeared luminous, and the anterior and posterior borders appeared cut after a model. On approaching the zenith there appeared many small clouds of much less elevation, but these were immediately united to the great mass, and without doubt were a part of it."

The following summary presents the positions that seem to be established by the data which have been obtained in reference to these two striking phenomena of the Gulf and the Texan Plains.

The southeast Monsoon of Texas is claimed to be a true *Desert Monsoon*, and analogous to the monsoons and desert winds of Africa and Asia.

The *Northers* are considered as peculiar to the physical structure of the continent and its relative position to the Gulf of Mexico, and not as having any necessary relation to the southeast winds, or to other districts than those in which they are felt.

The entire disconnection of both these phenomena from the changes occurring on the plains and mountains northward, and from the climate of the continent elsewhere, is particularly insisted upon, and their peculiarly local and limited character is traced to the position of the districts in which they occur, between the lines of marked effect in the system of general atmospheric circulation, and in latitudes which at sea would be a belt of calms.

As a true desert wind of aspiration, the southeast wind of Texas is a non-precipitating wind, increasing in temperature and rarefaction, and exhausting itself in a rainless district, beyond which it does not extend.

The Norther is regarded as a reverse wind of aspiration, the positive laws of which cannot at present be indicated, but some negative positions are taken as follows. It is not a wind of propulsion from the Rocky Mountains or from the Plains; — it is not a movement of the principal body of the atmosphere within the limits of cloud formation; — it is not a precipitating wind from its own volume, but when attended by storms, of any amount, has a nearly reverse current above it from which the principal precipitation takes place.

The Norther is considered to be entirely analogous to the northeast and southeast winds attending general storms in the United States. As in their case, the movement is one of aspiration, and the cause moves in a reverse direction above, thus giving the appearance of a movement *backward* at the surface, or contrary to the wind itself. The Norther, therefore, as the principal surface wind in the other storms alluded to, is a secondary phenomenon in all respects, and the remotest incident or attendant of great general disturbances.

Apparent contradictions of this position, and facts seemingly irrecon-

cilable with it, may have their solution in the peculiar place or point of initiation of the phenomena, which may be at any locality in the district of their prevalence.

An adequate cause of the most exaggerated of this class of phenomena may be suggested as existing in the intense heats and variable and excessive saturation of the mass of atmosphere of the western portion of the Gulf of Mexico. The maximum of these conditions may occur in spring and autumn, when also the adjacent lands attain the minimum of temperature and humidity. Sudden disturbances of the mutual relations of these contrasted volumes may, by ascent and displacement, produce the extreme of the effects designated as Northers. The peculiar conformation of the sea, and of the continent at these points, and the general relations of the districts to the trade winds, may render the inducing conditions also more irregular and extreme.*

Before this paper was read, Professor Henry rose and remarked that a large series of meteorological observations had been instituted by the Smithsonian Institution, by reason of the fact that Mr. Smith-

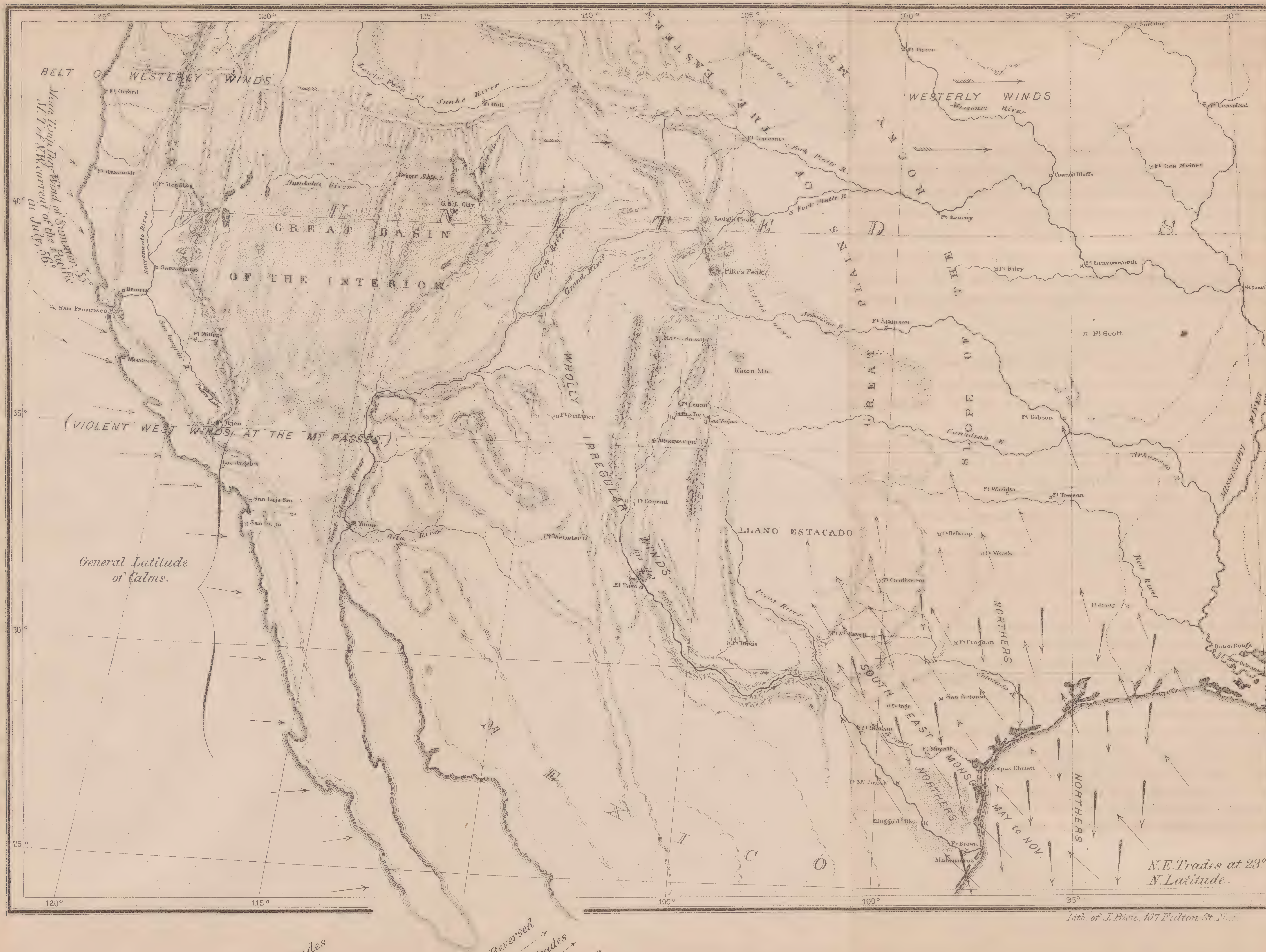
* From the Wind and Current Charts for the North Pacific, completed by Lieutenant Maury, since the first preparation of this paper, it appears that the northwest wind of the summer months on the California coast extends in a belt from Alaska to our coast at San Francisco, and, apparently central at the extremity of that peninsula and at San Francisco, covers an area three hundred miles in width and a thousand miles in length. It is so described by Lieutenant Maury. From the Wind and Current Charts, also, its temperature appears, from the mean of all observations in July, to be 56° for the first area of 2½° of longitude and 5° of latitude, adjacent to the coast of California at San Francisco; and for the next area of 5° of latitude and 5° of longitude westward, 61°; the third, 63° 5′; the fourth, 64°; the fifth, 68°, &c. Thus it is clear that the cold mass of this ocean is adjacent to the coast. Similar parallel areas farther south exhibit the same increase of temperature from the coast westward, and, though it increases in temperature southward, the sea in the vicinity of the coast is always much colder than in the Atlantic in the same latitudes. Other portions in this belt of winds are, unfortunately, not observed.

The Chart designates this mass as a "Northwest Current," but the surface scarcely exhibits any movement as a general current. It is, however, too great and massive a phenomenon to be secondary to atmospheric temperature, and it must have its partial solution, at least, in a movement of a mass of the water, perhaps beneath the surface, from the colder regions northwestward, and in conjunction with this peculiar belt of northwest winds.

E N. AMERICAN CONTINENT.

Lith. of J. Bien, 107 Fulton St.

CHART OF THE ABNORMAL ATMOSPHERIC MOVEMENTS OF THE N. AMERICAN CONTINENT.



Lith. of J. Bien, 107 Fulton St. N.Y.

son bequeathed his money for such purposes, in order to advance human knowledge. The investigations of Mr. Blodget had been made under the auspices of the Institution, and should be credited to it. Mr. Smithson was worthy of the honor, and all the papers relating to them should be regarded as emanating from the Institution. Mr. Blodget's observations were valuable, and as such should emanate from the proper promotive source.

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7. Indications of Weather, as shown by Animals and Plants. By W. H. B. Thomas, of Philadelphia.

The possibility of foretelling weather has occupied the attention of observers of natural facts from the earliest period of our record; the certainty with which anything is arrived at on this subject, like all other parts of natural science, depends upon the knowledge acquired of those things with which nature has most intimately connected it.

Without indulging in any comment, I will state a few particulars in regard to the different indicators with which nature has supplied us.

When a pair of migratory birds have arrived in the spring, they immediately prepare to build their nests, making a careful reconnoissance of the place, and observing the character of the season that is coming.

If it be a windy one, they thatch the straw and leaves on the inside of the nest, between the twigs and the lining, and if it be very windy they get pliant twigs, and bind the nest firmly to the limb, securing all the small twigs with their salivas. If they fear the approach of a rainy season, they build their nests so as to be sheltered from the weather; but if a pleasant one, they build in a fair, open place, without taking any of these extra precautions. In recording these facts, we have kept duly registered,—

1st. The name of the bird.

2d. The time of arrival in spring.

3d. The commencement of nesting.

4th. The materials of nests.

5th. The position of nests.

6th. The commencement of laying.

7th. Number of eggs in each nest.

8th. Commencement of incubation.

9th. Appearance of young.

10th. The departure in autumn.

But it is our insects and smaller animals which furnish us with the best means of determining the weather in advance.

We will now take the Helices, and show the various phenomena they present. These animals do not drink, but imbibe moisture in their bodies during a rain. At regular periods after the rain, they exude the moisture from their bodies.

We will take, for instance, the Helix Alternata. The first fluid exuded is the pure liquid ; when this is exhausted, it then changes to a light red ; then deep red ; then yellow ; and lastly to a dark brown. The Helix is very careful not to exude more of its moisture than is necessary. It might exude all of its moisture at once, but this is not in conformity with the general character of the Helix, as it would be too great an exertion.

The Helix Alternata is never seen abroad except before a rain, when we find it ascending the bark of trees, and getting on the leaves.

The Helices Arborea, Indentata, Ruderati, and Minuta, are also seen ascending the stems of plants two days before a rain.

The Helix Clausa, Ligera, Pennsylvanica, and Elevata, generally begin to crawl about two days before the rain will descend. They are seen ascending the stems of plants. If it be a long, hard rain, they get on the sheltered side of the leaf, but if a short one, they get on the outside. The Succinea have also the same habits, differing only in color of animal, as before the rain it is of a yellow color, while after it is a blue.

The Helices Solitaria, Zaleta, Albolabris, and Thyroideus, not only show by means of exuding fluids, but by means of pores and protuberances. Before a rain, the bodies of H. Zelata and H. Thyroideus have large tubercles rising from them. At the end of each of these tubercles is a pore. At the time of the fall of the rain, these tubercles, with their pores open, are stretched to their utmost to receive the water.

Also, for a few days before a rain, a large and deep indentation appears in the H. Thyroideus, beginning on the head between the horns, and ending at the jointure with the shell.

The Helices Solitaria, and Zaleta, a few days before a rain, crawl to the most exposed hill-side, where, if they arrive before the rain descends, they seek some crevice in the rocks, and then close the aperture of the shell with a glutinous substance, which, when the rain approaches, they dissolve, and are then seen crawling about.

In the Helix Albolabris, the tubercles begin to arise after the rain, while before they grew smaller, and at the time of the rain the body of the snail is filled with cavities to receive the moisture.

The H. Zaleta, Thyroideus, and Albolabris, move along at the rate of a mile in forty-four days. They inhabit the most dense forests, and we regard it as a sure indication of a rain to observe them moving toward an exposed situation.

The Helices Appressa, Tridentata, Fallax, and Paliata, indicate the weather, not only by exuding fluids, but by the color of the animal. After a rain the animal has a very dark appearance, but it grows of a brighter color as the water is expended, while just before the rain it is of a yellowish-white color. Also, just before a rain, striæ are observed to appear from the point of the head to the jointure of the shell. The superior tentacula are striated, and the sides are covered with tubercles. These Helices move at the rate of a mile in fourteen days sixteen hours. If they are observed ascending the cliffs, it is a sure indication of a rain. They live in cavities in the sides of cliffs.

The Helix Hirsuta is of a black color after a rain, but before it is of a brown, tinged with blue around the edges of the animal. The tentacula are marked by cross striæ, and there is also to be seen, a few days before the rain, an indentation, which grows deeper as the rain approaches. This Helix also exudes fluids, but not with the changes of color of those before mentioned.

We can also foretell a change of weather by the wasps and other insects.

The leaves of trees are, even, good barometers; most of them, for a short, light rain, will turn up, so as to receive their fill of water; but for a long rain, they are so doubled as to conduct the water away.

The Rana Bufo and Hyla are also sure indications of rain; for, as they do not drink water, but absorb it into their bodies, they are sure to be found out at the time they expect rain.

The Locusta and Gryllus are also good indicators of a storm; a few hours before a rain they are to be found under the leaves of trees,

and in the hollow trunks. We have many times found them thus, but we have never found the instinct of these little fellows to lead them to unnecessary caution.

B. CHEMISTRY AND NATURAL HISTORY.

I. CHEMISTRY.

1. On the Solidification of the Coral Reefs of Florida, and the Source of Carbonate of Lime in the Growth of Corals. By Professor E. N. Horsford, of Cambridge.

In a paper submitted to the Association in 1851, and subsequently, with some modifications, published in Silliman's Journal, the following general statements and conclusions in regard to the solidification of the Florida Reefs find expression : —

1st. That there are two kinds of solidified rock ; one coarse-grained and irregularly stratified, and another fine-grained, compact, and composed of numerous thin layers.

2d. That in the vicinity of the elevated reefs are depressions, which are filled at long and irregular intervals with sea-water, containing coral-mud and organic matter, including animal exuviæ, in suspension and solution, and which present, when nearly deprived of water by evaporation, a dark deposit, constituting in this place, it is believed by the writer, the parent of the fine-grained, compact crust-rock.

3d. That this deposit, collected and brought to Cambridge, was, after some weeks, still plastic, and yielded to water an excessively offensive smell, which water at first gave the reactions of hydro-sulphuric acid, at a later period yielded a film of carbonate of lime at the surface of the water, and was then of a decidedly alkaline reaction.

4th. That the hydro-sulphuric acid was due to the decomposition of the organic matter ; that more or less it slowly oxidated, leaving water and sulphuric acid ; that the sulphuric acid so formed decom-

posed pulverulent carbonate of lime, giving a soluble lime salt, and that this sulphate of lime was decomposed by ammonia evolved at a later period, yielding hydrate of lime and sulphate of ammonia.

5th. That the carbonic acid, evolved by the sulphuric acid, conferred solubility upon an undecomposed atom of carbonate of lime.

6th. That some of the excess of ammonia of the decomposing organic matter, after the reaction had become alkaline, decomposed sulphate of lime of the sea-water, forming hydrate of lime.

7th. That the hydrate of lime so formed, and carbonate, united to form the compound of Fuchs.

8th. That the solidification of the crust-rock was due in part to the formation of this compound, in part to the simple admixture of animal and vegetable matter acting like mucilage or glue, and still further, after the first series of changes had gone through, or when the reaction is continuously acid, to the slow production of carbonic acid from organic matter, which gave greater or less solubility to the pulverulent carbonate of lime, and permitted it to assume a more compact crystalline form.

9th. That on analysis the crust-rock was found to contain from 2.17 to 3.44 per cent of water, from 0.16 to 0.47 per cent of organic matter, and an inadequate quantity of carbonic acid to neutralize the base present, and that it contained neither ammonia nor sulphuric acid.

10th. That on analysis the soft rock, after air-drying for several months, was found to contain 20.16 per cent of organic matter, 2.05 per cent of sulphur, and 7.72 per cent of water.

11th. That "the exceeding fineness of the coral mud is due in part to the stone plants (calcareous vegetation of Dana) which flourish in the waters within the reef, and which admit of ready reduction to a powder of extreme fineness."

In view of the above it was remarked: "The chief conclusions to which the above research has conducted are,—

"I. That the submerged or oölitic rock has been solidified by the infiltration of finely powdered (not dissolved) carbonate of lime, increasing the points of contact; and the introduction of a small quantity of animal mucilaginous matter, serving the same purpose as the carbonate of lime, that of increasing the cohesive attraction.

"II. That the surface or crust rock has been solidified by having,

in addition to the above agencies, the aid of a series of chemical decompositions and recompositions, resulting in the formation of a cement."

In the same paper some suggestions are made in regard to the source of lime in corals.

12th. It was suggested that, since carbonic acid is not a constant ingredient of sea-water, the carbonate of lime of corals may be due to the decomposition of the sulphate present in sea-water with the exhaling carbonate of ammonia from coral animals yielding insoluble carbonate of lime on the one hand, and soluble sulphate of ammonia on the other. Several facts are cited which it is conceived lend support to the suggestion.

13th. The record of three specific gravities, made of portions from the centre, periphery, and midway between, of a mass of *meandrina* a foot in diameter, is given as a fact worth preserving, perhaps, for its physiological relations.

The above-cited paper has been reviewed by Professor Dana, the Geologist of the South Sea Exploring Expedition, in an article of some length in Silliman's Journal, in which he dissents from the views and reasoning of the paper.

I propose in the following paper to submit some new facts, tending to throw additional light upon the subject under discussion, and to reply to the arguments which Professor Dana has adduced in support of his peculiar views.

The first analyses of the crust-rock were made of specimens which, in general appearance, were not unlike, but which gave, in some respects, highly contrasted results.

These, individually considered, were satisfactory, although as a whole they failed to yield a hundred parts. As, however, the object in the analysis was qualitative rather than quantitative, — to ascertain *what* was present, and what in successive changes could have taken place, I did not hesitate to submit the results as they were. I remarked, however, in regard to the variability of the carbonic acid and water, that it might be due to the more or less advanced stages of change which the rock had undergone. Since the publication of the paper I have repeated the analyses with other specimens, and have found that the ingredient which escaped my recognition was *soluble organic matter*. Qualitative analysis gave the usual indica-

tions of lime, magnesia, soda, a trace of silica, carbonic acid, hydro-chloric acid, water, insoluble organic matter, and nitrogen; and the hydro-chloric acid solution had a faint reddish-brown color, which led to the recognition of *soluble organic matter.*

This body, which had before been unsuspected, accounted for the deficiencies of former analyses, and points us, possibly, to the nature of the *loss* so frequently recorded in analyses of secondary limestones.

The analysis of a fair specimen gave the following results: —

Determination of the Water.

I. 2.1877 gr. substance gave 0.0250 gr. water.
II. 3.4047 " " 0.0411 "

Carbonic Acid.

I. 2.4980 gr. substance gave 1.033 gr. carbonic acid.
II. 2.0178 " " 0.830 " "

Lime.

I. 3.2320 gr. substance gave 3.061 gr. carbonate of lime.

Magnesia.

I. 3.2320 gr. substance gave 0.090 gr. pyro-phosphate of magnesia.

Chlorine.

I. 4.2100 gr. substance gave 0.131 gr. chloride of silver.

Insoluble Organic Matter.

I. 3.2320 gr. substance dissolved in hydro-chloric acid gave 0.008 gr. residue.
II. 4.2100 gr. substance dissolved in nitric acid gave 0.0120 gr. residue.

Organic Analysis.

I. 2.4338 gr. substance burned with chromate of lead gave 1.0730 gr. carbonic acid and 0.0534 gr. water.
II. 2.7680 gr. substance burned with oxide of copper gave 0.0460 gr. water.
III. 1.7046 gr. substance with soda lime gave platino-chloride of ammonium, which left 0.0280 gr. of metallic platinum.

Estimating the sodium from the chlorine, with which it was combined, we have of sodium 0.49 per cent.

Estimating the nitrogen from the metallic platinum, we have 0.23 per cent, and conceiving this to have the constitution of albumen, we

have of albuminous matter 1.47 per cent, which requires 0.806 per cent of carbon and 0.102 per cent of hydrogen. There were found 0.775 per cent of carbon and 0.185 and 0.186 per cent of hydrogen. As, however, the total nitrogen cannot be supposed to have been in the form of albumen, but some of it in some form nearer to ammonia and more soluble, so also the carbon was some of it in the form of cellular tissue, and we shall probably be as near the truth as the case admits, if we omit the insoluble organic matter 0.28 and 0.25 per cent, which was doubtless in some degree cellular tissue, and accredit all the soluble and insoluble organic matter as albuminous matter. We shall then have expressed in per cents: —

	I.	II.
HO	1.14	1.21
CO_2	41.35	41.13
CaO	53.03	
MgO	1.02	
Cl	0.76	
Na	0.49	
Albuminous matter	1.47	
	99.26	

In the specimen here analyzed the stage of decomposition of the organic matter was further advanced.

The former analyses gave for the minimum of

Carbonic acid	34.01 34.38
Maximum of the same	38.89 38.94
Minimum lime	51.17
Maximum lime	53.12

The minimum of lime requires of carbonic acid	40.20	per cent.
Analysis gave (average)	34.19	"
Excess of lime	6.01	"
The maximum of lime requires of carbonic acid	41.71	per cent.
Analysis gave	38.91	"
Excess of lime	2.80	"

In the more recent analyses there was found, beside lime, a small

percentage of magnesia. If we unite these bases they require for their neutralization,

Of carbonic acid	42.79	per cent.
Analysis gave (average)	41.24	"
Excess of base	1.55	"

It is obvious, therefore, in accordance with the suggestion above, that the changes of organic matter had further advanced.

Professor Dana, in the paper above cited, urges that the carbonic acid is adequate to the neutralization of the lime recorded, assuming that the direct determination was defective, and that the excess of total volatile matter over the determined organic matter and water should be regarded as carbonic acid. Neither my subsequent determinations, nor any other observations, have led me to doubt the accuracy of the earlier carbonic acid results; and this excess of volatile matter is manifestly due to the soluble organic matter, as shown by the subsequent analyses. What is said of the analyses as a whole I will notice further on.

It is further remarked, in view of the writer's conceived succession of changes which the mud-rock undergoes: —

"How can organic matter, including carbon, oxygen, hydrogen, nitrogen, and sulphur, where this matter is in progress of decay, evolve *pure* [free] sulphuretted hydrogen, and not hydro-sulphuret of ammonium? But suppose that the pure sulphuretted hydrogen is evolved, would it, under the circumstances, change to sulphuric acid? Suppose it to change to sulphuric acid, and this to combine with lime to form the sulphate of lime, as the theory states: is it then possible that *uncombined ammonia* should be formed from animal matter containing the elements of both carbonic acid and sulphuretted hydrogen? Such a subversion of chemical laws can hardly be expected, even for the benefit of the coral reefs of Florida. But suppose the uncombined ammonia to be formed during the decomposition: will this ammonia *precipitate the lime from the solution of the sulphate*?"

The first division of the argument, if I understand it, is, that in a decaying mass of organic matter, containing carbon, hydrogen, oxygen, nitrogen, and sulphur, the evolution of *free* sulphuretted hydrogen is impossible; and further, that from any organic mass containing the above ingredients at any time the evolution of uncombined ammo-

nia is alike impossible. In other words, from the commencement to the close of the decay, its conditions must be neither acid nor alkaline, but neutral. Upon these points there is a difference of opinion between the critic and other authorities.

I have proceeded in my explanation upon what I regarded as settled principles of chemical science, with regard to fermentation and decay of animal matter; — that, with the first absorption of oxygen, acid products are formed which in their order of volatility are expelled; that the period of acid reaction depends upon the relative proportion of bodies allied to starch, sugar, or woody fibre, as compared with the nitrogenous compounds; that in organic masses containing much albumen, or bodies allied to it, the alkaline reaction would set in sooner or later; and that if hydrate of lime existed in a mass where originally there were only sulphate and carbonate, mingled with nitrogenous matter, it could only be due to the production of ammonia. Mr. Dana regards this view as erroneous, and his view is given above.

It may be well, before citing any authorities, to present some facts which had escaped the recollection of the Geologist of the Exploring Expedition: as, for example, the effect of fermentation and decay on gluten or dough, where acetic acid is a uniform product; or on some varieties of cheese, which with age take on an alkaline reaction.

He seems not to be aware that sulphuretted hydrogen is ever evolved from decaying organic matter in or about the sea. The well-known occurrence of sulphuretted hydrogen in the Bay of Marseilles since the year 1830, and the attendant death of the fish in the waters, seem to have escaped him. The explanation of Blanchet,* that sulphates in the waste waters of neighboring manufactories flowing into the sea were reduced by organic matter and sulphuretted hydrogen therefrom made free, seems alike to have been absent from his mind.

This is not the only instance. Dr. Burtt, U. S. N.,† has published an able paper upon the destruction of fish by sulphuretted hydrogen, in the Bay of Callao.

But let us look at the authorities bearing upon the point.

The first point is, Can a decaying organic mass, containing carbon,

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* Liebig's Ann., LIX. 109.

† Proc. Acad. Nat. Sci., Philadelphia, VI. 1.

hydrogen, oxygen, and sulphur, yield, in the process of its decay, uncombined acid products?

If there be present in an acid solution fixed and volatile acids, the latter will be more or less free. In a solution containing lactic acid, butyric acid, or acetic acid, and also hydro-sulphuric acid and carbonic acid, if any base were present, it would be, in larger measure, appropriated to the more fixed, in smaller measure to the more volatile acids.

Saussure made numerous experiments with peas, beans, wheat, rye, barley, and other seeds, and found that in decay they evolved chiefly carbonic acid gas.

Erdmann and Marchand * found that wheat yielded carbonic acid gas, and hydrogen free from carbon. The fluid contained butyric acid. White beans evolved carbonic acid gas through a period of nine weeks, and the fluid then contained ammonia and butyric acid. Peas yielded at first carbonic acid gas, then a mixture of the same with hydrogen, and traces of sulphuretted hydrogen. They also yielded butyric acid, which yielded in eight weeks only a little carbonic acid gas, no hydrogen, and only a trace of butyric acid.

Milk exposed to a limited quantity of air after a time became acid, and remained so after distilling volatile products, showing that carbonic acid was not the only acid produced.†

The second point is, Can an organic mass, containing carbon, hydrogen, oxygen, and sulphur, yield free ammonia?

1. According to John Davy, blood, fibrine, cerebral substance, muscle, liver, spleen, and glands yield carbonic acid in the first twenty-four hours. At a later period follows, with the evolution of carbonic acid, ammonia, and sometimes hydro-sulphuric acid.

2. I have made the experiment of spontaneous decay of the brain, kidneys, muscles, and lungs of a bear, and found them all to yield hydro-sulphuric acid and carbonic acid to solutions placed in the pathway of evolved gases. At a later period, the evolution of hydro-sulphuric acid ceased, and all the masses became alkaline.

3. Provost states that cheese and gluten under water evolve offensive gases, impart to the water free acid, with hydro-sulphuric acid, and thereafter carbonate of ammonia.

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* J. P. Chem., 29, 465.

† Th. v. Dusch. and Gmelin's Handbuch der Chemie, IV. 93.

4. The solution of glue is well known to go over into ammoniacal putrefaction without the preceding acid stage.*

As early an author as Fourcroy remarked that muscle went into putrefaction with an intolerable smell of ammonia. In most of the other instances cited, the alkaline reaction succeeds an acid reaction.

The mass in question contains, beside the decaying animals and animal exuviæ, a certain amount of vegetable matter, which, in its decay, would tend to give to the mass a prolonged acid reaction, while the animal matters would tend at a later period to give it an alkaline reaction.

We have in the alimentary canal a somewhat similar condition of things. The contents of the stomach react acid; this is also true of the contents of the duodenum and jejunum, but in the ilium the acid reaction begins to diminish, so that often, a long distance before reaching the cœcum, the acid reaction is quite lost. From secretions in the cœcum it is sometimes acid, but uniformly the contents of the colon are alkaline.†

The juice of meat has an acid reaction; ‡ it rapidly assumes in decay the alkaline. The juices of vegetables, with scarcely an exception, left to decay, become acid. A mixture of the two in the alimentary canal would, independent of the gastric juice, be at first acid. At a later period, notwithstanding the acid contributions from various sources, it becomes, in the form of excrements, alkaline.

The excretions of the kidneys, though uniformly acid when first discharged, rapidly become alkaline.

It were needless to examine the facts and authorities further.

The conditions of the decaying mass which was the subject of my examination, including *the earlier acid character*, *the subsequent alkaline reaction*, *the formation of the film of carbonate of lime at the surface*, *and the existence of an excess of base in the crust-rock*, seem to me to admit of no other explanation than that I have given.

It is further doubted whether, if hydro-sulphuric acid were evolved, it would yield water and sulphuric acid. The conditions are these: a mixture of organic matter, more or less advanced in decay, coral

* Gmelin's Handbuch der Chemie, IV. 90.

† Lehman, Phys. Chim., II. 114.

‡ Chem. of Food, Liebig, Lowell edition, p. 23.

mud, and water; the water slowly evaporates, and the whole is exposed to air. This represents the condition of the decay of simple animal matter, in which the sulphur is known more or less to become sulphuric acid.

Professor Dana proceeds: "But suppose the uncombined ammonia to be formed during the decomposition, will this ammonia precipitate the hydrate of lime from the solution of the sulphate?" It is, perhaps, sufficient to say of Professor Dana, in regard to this statement, that it was written in entire misapprehension. It is due to myself, however, to add, that I have never uttered anything of the kind, and that, in an earlier allusion to the view, the learned author correctly states what I did say. He says, in repeating my views: "The ammonia resulting from the nitrogen carries off the sulphuric acid as sulphate of ammonia, and leaves the lime as a SOLUBLE *hydrate*."*

The conditions in which I conceive hydrate of lime to have been produced, and from which I conceive it to have been withdrawn combined with carbonate as the Fuchs compound, are these.

The organic matter in its fermentation and decay at first yielded acid products. I have noticed only two. These may be regarded as types representing the condition of the mass, for in similar masses there are found lactic, butyric, and acetic acids, and there are doubtless others. These acids decompose some of the carbonate of lime, and yield soluble salts. Hydro-sulphuric acid, as such, would give solubility to carbonate of lime. The sulphuric acid, resulting from the oxidation of hydro-sulphuric acid, would give sulphate of lime. At a later period ammonia is found, which, uniting with the acids, forms salts of ammonia, leaving the lime as a soluble hydrate; the excess of ammonia, with the gradual evolution of carbonic acid, keeps the hydrate and carbonate in readiness to combine with each other.

That the Fuchs compound would result from this condition of things, is as obvious as that it takes place in a mortar prepared from hydrate of lime. In the analysis of limestone of much earlier date than the coral reefs of Florida, it is not unusual to find combined water. I cite an example in which magnesia occurs.

J. Roth† gives, in a marble from Pedrazza, in Tyrol,—

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* American Journal of Science, [2] July, 1852, p. 83.

† Jour. f. Prakt. Chem., Band LII. s. 346–353.

HO	6.96	7.00
CO_2	33.35	33.98
CaO	44.67	42.63
MgO	14.54	14.05
$Si\,O_3$, Al_2O_3, Fe_2O_3	.48	.78
	100.00	98.44

and for it the formula, $2\,[CaO, CO_2] + MgO, HO$.

In what condition is this water?

Another, by the same chemist, gave,—

HO	10.92	10.97
CO_2	29.23	28.10
CaO	35.70	35.97
MgO	24.78	24.47
	100.63	97.51

and the formula, $CaO, CO_2 + MgO, HO$.

The water driven out by heat in the analysis contained *ammonia.* Another variety, from the same locality, gave of

HO	10.89	10.50
CO_2	25.00	26.40
CaO	35.42	38.47
MgO	24.32	24.64
Fe_2O_3, SiO_3	1.05	1.05
	96.68	98.06

For this the above formula nearly applies.

Lesser quantities of combined water are noted in several analyses that have fallen under my eye. A long series quoted in Knapp's Technology may be referred to as exhibiting water in chemical combination.

The general doctrine of a series of decompositions and recompositions, in which carbonate of lime and organic matter play a part, resulting in the formation of a cement which I have advocated, is regarded as altogether new.

Some results of scientific investigation in this line have escaped Professor Dana's notice. Kuhlman* ascribes to the action of the alkalies potassa and soda, in limestones of different geological epochs, and also in many other rocks, a process of cementation which con-

* Comptes Rendus, XXIV. 263.

nects itself with the earlier researches of Fuchs.* The action in the cases this author cites is quite the same as that of ammonia would be.

Marcel de Serres and Figuier,† in a research of much labor, remark as conditions of petrifaction, that the animal matter be under water, and that the water contain, in solution, salts of lime or silicates, according to the kind of material which is to replace the animal matter. They had studied the law of the phenomenon in its general conditions.

It is not to the formation of this hydrate alone that I have ascribed the solidification. Two other instrumentalities are enumerated in my paper. There is the deposition of finely powdered carbonate of lime with mucilaginous matter filling up the interstices between the grains of rock, and serving to increase the cohesion, — a process like that of cementation by means of albumen and powdered lime or chalk. Of this Professor Dana says: "Infiltration of finely powdered (not dissolved) carbonate of lime and mucilaginous matter can hardly be admitted as the means of solidifying the submerged oölitic rock."

In an elementary work on Geology, by Loomis, occurs, at page 119, the following sentence: "Some rocks are composed of such materials, that they set like hydraulic cement where they are deposited."

If any time should be devoted to a reply to this criticism, it may be added, in a word, that the action of glue is of the same character.

Let us now consider the causes to which Professor Dana ascribes the solidification of the crust-rock. He remarks: "The eminences of drift sand-rock at the Sandwich Islands were covered, in part, by a smooth solid crust, two or three lines thick, and made of layers like

* In the Jahrsbericht of Liebig and Kopp, for 1847–48, the following remark occurs at page 1241: "Die Untersuchungen Kuhlmann's uber die Gegenwart von Käli oder Natron in den Kalksteinen der verschiedenen Geologischen Epochen, namentlich in den hydraulischen Kalken sowie in vielen anderen Gesteinen, die sich den älteren trefflichen Erfahrungen von Fuch's anschliessen, sind auch von Geologischem Interesse indem sie die allgemeine Gattung des *Cämentbildungsprocesses* bei der Bildung und Erhärtung von Felsarten und Mineralien darthun, und die Art der Agglutination der Conglomerate, Breccien, u. s. w., durch sichere chemische Erfahrungen erlautern und versinnlichen."

† Jameson's Journal, XLIV. 50. Phar. Cent., 1848, 257.

stalagmite, which was formed by *the solution of lime from the surface by the rains, and its deposition again on evaporation.*" *

Of this rock, as occurring in the Pacific, the author further remarks: "The crust simply covers some very small portions of the surface, conforming to its undulations and not to the lamination of the rock itself, and occurring especially in depressions." †

I have already remarked, in a note to the paper published in Silliman's Journal, that "this would account for its occurrence in depressions of the rock, but would not account for its occurrence on eminences or abrupt slopes, nor would it account for the presence of water as hydrate of lime." ‡

Two plain considerations will show that the opinion of Professor Dana was a generalization upon insufficient data: —

1. In Florida the crust-rock, in some instances, covers areas of acres in extent, surrounded by elevations in most cases of very moderate height. It is made up of a series of thin layers. Now, admitting the lowest layer to have been dissolved by rain-water, and again deposited, whence was derived the material for the next layer above, and the next, and so on to the top.

2. There occur in the coarse-grained rock of Florida, dome-shaped cavities open toward the bottom to the sea, but which the rain never reaches, and these are invariably lined with the crust-rock.

If the explanation of the learned author be correct, we may ask why the beds of chalk in the Old World, and the beds of calcareous marl in the Old and the New, are not crusted without, from the action of rain-water.

The same author ascribes the solidification of the drift sand-rock, which I have spoken of as of oölitic structure, to the solution of the carbonate of lime by sea-water, and its deposition upon and between the grains of the rock.§

The author says: "The cementation of coral sand along shores and beneath the sea is illustrated among all reefs, and is the process by which reef-rocks are formed. The sea-water receives some carbonate of lime into solution, and again deposits it among the deposited sand and fragments which lie compacted together."

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* American Journal of Science, [2] XIV. 81.

† American Journal of Science, [2] XIV. 411.

‡ American Journal of Science, [2] XIV. 251.

§ American Journal of Science, [2] XIV. 81.

The author further remarks: "The waters of the sea have been found to contain a small proportion of *free carbonic acid*, which is sufficient to enable it to dissolve the carbonate of lime of the corals."* The confidence in this statement abates somewhat at a later period, as we find the author remarking: "Facts show that there is not a total want of carbonic acid (or carbonate of lime) in sea-water about coral reefs.†

This is more in keeping with the remark of Bibra, who has made the most elaborate analyses of sea-water on record, that the reaction of sea-water is, although slightly, *invariably alkaline*.‡

The author again remarks: "The possibility of . . . . . the presence of carbonic acid in sea-water is denied by Professor Horsford."§ It may be well enough to quote here what I did say. In speaking of the source of lime in the growth of corals, I go on to say: "Marcet,‖ as early as 1823, observed carbonate of lime in the sea-water near Portsmouth. Jackson¶ found it in two specimens of sea-water furnished by the United States Exploring Expedition, one from 600 feet, and the other from 2,700 feet below the surface. J. Davy** found the sea-water of Carlisle Bay, Barbadoes, to contain about one ten-thousandth part of carbonate of lime. There was found scarcely a trace near the volcanic island of Fayal. White†† is of the opinion that it fails only near the surface; but the elaborate analysis by Bibra,‡‡ of no less than ten specimens, taken generally from a depth of twelve feet, but in one instance from a depth of four hundred and twenty feet, in various latitudes on both sides of the equator, shows quite conclusively that it is not a constant ingredient of sea-water. His analyses do not mention a trace of carbonate of lime. The quantity found by Davy is very nearly that which is soluble in water, and is obviously due to the calcareous mud which abounds near the Barbadoes. The water from within the Keys was carefully ana-

* American Journal of Science, [2] XIV. 81.

† American Journal of Science, [2] XIV. 416.

‡ Liebig's Annalen, LXXVII. 94.

§ American Journal of Science, [2] XIV. 412.

‖ Annals of Phil., April, 1823, 261.

¶ American Journal of Science, [2] V. 47.

** Phil. Mag., [3] XXXV. 232.

†† Phil. Mag., [3] XXXV. 308.

‡‡ Liebig's Annalen, LXXVII. 94.

lyzed in my laboratory; it contained lime and sulphuric acid among its ingredients, but not a trace of carbonic acid."

Professor Dana records its occurrence. He quotes, among others, Schweitzer's analysis of sea-water in the British Channel, which gives three parts of carbonate of lime in one hundred thousand of the water.

It may be asked, according to Professor Dana's view, why all beaches, bathed and saturated by sea-water, are not solidified by this carbonate of lime which it holds in solution.

Marcel de Serres and Figuier* have suggested, in the paper already quoted, a process which, in the presence of soluble carbonate of lime, would undoubtedly play a part. It is illustrated in crystallization, where an already formed crystal is thrown into a saturated solution. The salt is withdrawn from solution more rapidly than it would be but for the presence of the crystal. M. de S. and F. call it a kind of central attraction.

I am persuaded that a process, to which I have alluded in my paper, is of much higher importance in the consolidation of the sand-rock, and fulfils an office in the solidification of the crust-rock of no inconsiderable moment. It applies especially to those instances where the reaction is acid throughout, and doubtless, to a less extent, where the reaction is for a time alkaline.

In the latter case, after the first series of changes has been completed, that is, after the acid and alkaline stages have been passed, and resulted in the formation of more or less of hydrate, and the rock has attained a degree of consolidation, there still remains, not much nitrogenous matter, but more or less organic matter that, by oxidation, will furnish water and carbonic acid. To this slowly evolved carbonic acid, both in the coarse-grained and fine-grained rock, I refer in the following paragraph: —

"To the chemical changes are added . . . . . the decomposition of the organic matter furnishing carbonic acid, which gives solubility to the pulverulent carbonate of lime."†

The slowly evolved carbonic acid would give to surrounding pulverulent carbonate of lime, if not perfect solution, a degree of mobility which would permit the larger grains of carbonate of lime to attract it to themselves, and thus yield a kind of cement. This process has

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* Pharm. Cent. Blatt, 1848, 257.

† American Journal of Science, [2] XIV. 249.

a wide range of illustration, and lies, I am strongly persuaded, at the foundation of the origin of the numerous more or less crystalline septaria that occur in some of the New York slates.

A mass of organic matter imbedded in a deposit at the bottom of an ancient ocean, after a time, with the absorption of oxygen from water in solution, slowly yields its products of decomposition, for the most part acid. Although some higher organic acids, and some ammonia, would result under such circumstances, the larger measure of ultimate products would be water and carbonic acid. The carbonic acid would give mobility to the carbonate of lime, which would take on, according to the perfection of the solution, more or less of crystalline form. "If the carbonic acid of carbonated water holding carbonate of lime in solution be permitted slowly to escape, the carbonate of lime assumes the crystalline form."* The greater density thus attained would provide cavities. The perfection of the solution or mobility of particles would diminish with distance from the source of the carbonic acid, and the size of the septarium would be, other circumstances being equal, proportioned to the magnitude of the organic mass. Another illustration of the process is found in the conditions presented by the numerous univalve, bivalve, and many-chambered fossil shells and fossil corals of the calcareous slates of the Silurian rocks of Western New York. They are, in many cases, resolved into spar. The organic matter they once contained has, in its decay, provided for the mobility of the previously organized or amorphous carbonate of lime, and permitted it to assume crystalline texture.

It may be worthy of inquiry whether this condition would not permit the replacement of the lime to some extent by magnesia, where the rock is penetrated by sea-water containing chloride of magnesium in solution, and thus give rise to dolomite. The process thus illustrated on a scale of considerable magnitude is, I conceive, exemplified as one of the processes in the solidification of the Florida reefs.

I should add, that in my earlier paper I had conceived the application of this process only to the crust-rock. I am now satisfied that it plays an important part in the solidification of the coarser rock.

Upon an examination of a specimen of the crust-rock, and of that which I conceive to be its parent, now, after the lapse of two years

* Mitscherlich's Lehrbuch der Chemie, 1847, Band II. s. 136.

and a half from its collection, when it was a plastic mass, no doubt will exist as to their relation to each other.

II. The surface of the reefs, and the waters within them, abound in stone plants (calcareous vegetation of Dana), which admit of ready pulverization. As the upper portions of the reefs are, upon their appearance at the surface of the sea, broken off by the joint action of the rock-borers and the waves,* and dashed against each other, resulting in the production of fragments of various sizes, from the coarsest blocks to an impalpable powder, it is obvious that the plants, more frail than the corals, must pulverize more readily, and contribute a share to the coral mud. I remark : "The exceeding fineness of the coral mud is due, in *part*, to the stone plants which flourish in the waters within the reef, and which admit of ready reduction to extreme fineness."†

Professor Dana, in noticing this view, quotes the above sentence literally, except that he omits the words "*in part*," upon which alone all the point of the sentence depends. It is unfortunate that we find the author elsewhere remarking: "The nullipores, properly calcareous vegetation‡ (stone plants), flourish best along the line of breakers, and form *thick accumulations upon the reefs*." It is enough to say that this was done in haste. Professor Dana proceeds : "The correct explanation, as has appeared to the writer,§ is, that it is owing to the trituration of whatever coral fragments and shells may be deposited in places where the motion of the water is gentle, while the coarser sand and pebbles are found where the waters are subject to more violent agitation, as in the face of the breakers." The entire confidence of the author in this view is evinced in the sentence that follows: "The same would happen under like circumstances if the rock material were granite instead of coral." Elsewhere the author remarks: "The rude sea-shore waves give rise to sand or pebbles, while the gentle undulations or ripplings of inland water produce mud by their finer trituration."

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* Professor Agassiz, before American Academy.

† American Journal of Science, [2] XIV. 249.

‡ The printing of *Millepora* for *Nullipora* in the manuscript has led Professor Dana into some intimations to which it is only necessary thus to allude.

§ Wilkes's Expl. Exp., Geol. Rep., p. 157, and American Journal of Science, [2] XII. 333.

Expressed in other terms, the doctrine would be this. The fineness is inversely proportioned to the agitation of the water; or the less the agitation, the finer the coral powder resulting. One may ask, What degree of fineness would be attained where the water is absolutely at rest? Ought we in the depths of inland lakes ever to find a gravel bottom, instead of one where all the drift is ground to powder by the gentle undulations or ripplings?

The facts I conceive to be these: the pulverization is most rapid where the agitation of the coral blocks, larger or lesser, is greatest. The coral mud is the fine portion of the products of trituration, separated from the coarser grains by currents, and deposited farther from the source, because more easily borne by the currents. It is, in short, I conceive, the result of a grand process of elutriation. The coral mud, like the refined clay of a porcelain establishment, is the most distant and the latest deposit. It will be seen, upon examining the stone plants and a mass of coral, how much more frail are the former, and how inevitably they must contribute, *in part*, to the fineness of the coral mud.

III. A mode of derivation of the carbonate of lime of coral was suggested in my paper. It is well known that sulphate of lime is a constant ingredient of sea-water; that from living animal tissues carbonic acid and ammonia are being constantly secreted, as products of decomposition, is a settled principle of physiology. It is equally well settled as a chemical principle, that when carbonate of ammonia and sulphate of lime in solution come together, there is carbonate of lime formed. In view of these facts, it was suggested that the carbonate of lime of the coral was the fruit simply of double decomposition of the sulphate of lime in sea-water by the carbonate of ammonia exhaled from the living coral.

From this view Professor Dana dissents. I am willing to leave the suggestion as a hint in research bearing upon this point. I will, however, add a few facts of interest in this connection.

From an investigation of recent hydraulic limestones, Damour* comes to the conclusion, "that the plants and the polyps decompose the soluble magnesia and lime salts of the sea, that after the change into carbonates these are taken into the organism, and that the for-

* Bull. Geol., [2] VII. 675.

mation of many magnesian limestones may in this manner be explained.

Two recorded analyses of limestones give, —

	$CaO.CO_2$.	$MgO.CO_2$.	NaO.	KO.	Fe_2O_3.	SO_3.	PO_5.	Org. mat.	Qtz. sand.	HO.	Total.
I.	87.32	8.51	0.45	0.34	0.55	0.89	0.23	0.35	0.63	0.64	99.91
II.	77.36	11.32	0.55	0.25	0.08	0.95	0.32	4.70	1.36	1.46	98.37

The journal containing these statements had not reached me when my previous paper was sent to the press.

The pearl, the production of which, as is well known, is not confined to the pearl-oyster, has been regarded as an abnormal growth, and due to some organic derangement. Irritation of a particular part is attended with increased action, increased decomposition, and increased evolution of carbonate of ammonia, and this, occurring in a fluid containing a large percentage of sulphate of lime, cannot fail to precipitate carbonate of lime. Such pearls we have occasionally in lobsters, and a more remarkable instance of it has recently been brought to the attention of the Natural History Society in Boston, in fishes from a lake in Vermont, where scales and fins and tail are more or less gemmed with delicate pearls. Although I have not had opportunity to examine the water, I feel that there is great probability that the water of that lake will be found to contain lime salts in unusual percentage for lake-water.

Professor Dana remarks of this process: "Such a mode of growth by accretion would be indicated by the absence of all cellular structure, and by a crystalline texture in the deposition. But a thin slice of coral, examined with the highest magnifying powers by polarized light, shows, as the writer has observed, no distinct crystalline texture."

On the contrary, it appears to me that the precipitate of carbonate of lime, under the circumstances supposed, would take place in the walls of the cellules where the carbonate of ammonia is set free, and would of course take the form of the cellules. And further, that the absence of crystalline texture in the carbonate of lime is precisely what might have been expected.

Professor Dana goes on to remark: "If coral zoöphytes require the medium of carbonate of ammonia, so must also the mollusca, as they derive their carbonate of lime from the same waters; and so, also, fishes and cetacea, except so far as they may receive it from animals

taken as food. The process is essentially alike in all." This is my persuasion. We have yet to be informed whence the oysters and numerous shell-fish derive their carbonate of lime, if not from the carbonic acid exhaled from their own organism and the lime as a salt in the sea, the former having parted with its ammonia to the acid combined with the lime. I have added at the conclusion of my first paper the remark: "The above research . . . . . lends support to the suggestion, that the carbonate of lime of corals is derived from the sulphate in sea-water by double decomposition with the carbonate of ammonia exhaled from the living animal."

IV. There is another point upon which the views of Professor Dana are peculiar. He remarks: "Among the modes of consolidation of limestone or carbonate of lime, the following are enumerated by Professor Horsford. 1. The common mortar process. 2. That of hydraulic cements. 3. That of deposition from calcareous springs. Of the last he says: 'The waters containing carbonate of lime held in solution by an excess of carbonate of lime, upon reaching the surface under less pressure and the influence of a high temperature [*higher* temperature in the manuscript, E. N. H.], give up the carbonic acid, so that a precipitation of carbonate of lime takes place.' But is this pressure or a higher temperature needed? The common deposition of carbonate of lime from the waters dripping through the roofs of caverns is evidence to the contrary."

I quote the whole literally, with its various inaccuracies, to illustrate the haste with which the article was penned.

The pith of the criticism is the gratuitous intimation that diminished pressure and elevated temperature are not required for the escape of carbonic acid in order to the precipitation of carbonate of lime held in solution by carbonic acid.

When soda-water, relieved of pressure, is discharged into a glass, we have a copious evolution of carbonic acid gas. The water percolating through limestone, and carrying with it carbonate of lime in solution, upon reaching the surface, as in the case mentioned by Professor Dana, finds itself surrounded by the *atmosphere*, while before it was surrounded by rock. Before, there was no space into which the carbonic acid could escape. Varying with the thickness of the saturated rock constituting the roof of the cavern, the pressure was the pressure of a column of water increasing with the height of the

column. It may have been many times the pressure of the atmosphere. On emerging, this pressure became the pressure of the atmosphere only.

In Silliman's Chemistry occurs the following remark: "Cold water recently boiled absorbs rather more than its own volume of carbonic acid gas, but with pressure more will be taken up, in quantity *exactly proportioned to the pressure exerted.*"*

As to the influence of temperature, it seems unnecessary to quote authority upon a matter familiar to students in chemical manipulation. In Geiger we find the following: "Most spring-waters become turbid on boiling with the deposit of carbonate of lime. Exposure to the air, or heat, or addition of lime-water, causes the evolution of carbonic acid, and the dissolved carbonate of lime is precipitated." †

Gmelin says: "The solution of carbonate of lime in carbonated water exposed to the air, or heated to boiling, evolves carbonic acid, and permits neutral carbonate of lime to fall." ‡

V. There remain to be considered the analyses which have shared with the several points of my paper the criticism of the learned author. He says: "The analysis was made, as appears from the account, out of sixteen different portions of the material, and of the seven ingredients contained, no two were estimated out of the same portion. This method, however satisfactory for homogeneous substances, will not answer for heterogeneous substances, where every portion as found differs widely from others in the proportions of the ingredients."

The intimation is, that the sixteen portions employed were each, as compared with the fifteen others, heterogeneous, and that all the determinations should have been made from a *single portion.*

To appreciate the value of this criticism, it may be remarked that it is generally considered that repetition increases the value of determinations; and further, that four carbonic acid determinations cannot be made from the same portion; and still further, that the portion employed for the determination of carbonic acid cannot be employed for the determination of either the water or total volatile matter; nor can those employed for the water or total volatile matter be employed for the organic matter. When I add, that the determinations throughout are duplicate, and in the case of carbonic acid fourfold, and that

* Ed. 1852, 222.

† Liebig's Geiger's Handbuch der Chemie, 1843, 428.

‡ Band II. 187.

there were but *two different portions*, *each* rendered homogeneous before use, it will probably be a sufficient reply to the intimation that sixteen different portions of substance were employed.

It can scarcely be necessary to follow the remainder of Professor Dana's criticisms. Two only will I notice. In speaking of the amount of sulphur in my analyses, the author says: "In the first place 20 per cent contains 0.3 per cent, or one seventieth of its weight, of sulphur." I have stated that "the mass digested in hydro-chloric acid gave of sulphate of baryta as follows:—

I. 2.3380 gr. gave 0.1040 gr.
II. 1.5325 gr. gave 0.1304 gr.

The organic matter by itself, oxydated, &c., yielded sulphate of baryta of which 1.5325 gr. gave 0.1505 gr.," taking, it will be observed, the same weight of original material, and giving to understand, with my summing up below, that it was the organic matter of the mass.

I state further as follows: "The following per cents are estimated upon the *substance* dried at 100° C.

Sulphur existing as sulphate and soluble in dilute hydro-chloric acid: average	0.94	per cent.
Sulphur in organic matter	1.45	"
Total sulphur of the above determinations . .	2.39	"

The sulphur determined by direct oxidation of the whole mass gave a smaller amount, 1.72 per cent, and an average total of 2.05 per cent. My record of analyses states: "The whole mass oxidated in a mixture of fused nitrate of potassa . . . . . gave of sulphate of baryta as follows:—

I. 2.3090 gr. gave 0.2850 gr.
II. 1.4322 gr. gave 0.1550 gr.

which correspond with

I. 1.61 per cent, and
II. 1.84 per cent of sulphur."

Professor Dana further remarks: "In Professor Horsford's second column 2.05 is not the amount found with 20 per cent of organic matter, but with 100 per cent, and hence the discrepancy."

To appreciate the care with which the paper was read, let us adopt this mode of construction.

The first sulphur determination recorded is of the portion *not* con-

tained in the organic matter, of which two determinations* were made, yielding an average of 0.94 per cent.

Now if we deduct this from the total sulphur determined by oxidation of the total mass, we shall have that due to the organic matter.

Total sulphur as soluble sulphate and inorganic matter	1.72 per cent.
Sulphur as soluble sulphate	0.94 "
Sulphur in organic matter	0.88 "

Thus 0.88 per cent of the whole is due to 20.16 parts of organic matter. To 100 parts of organic matter would, therefore, be due, according to Professor Dana's reading, 4.36 per cent.

Professor Dana remarks above, that the animal matter, according to the determinations, contains but *one seventieth* of its weight of sulphur. Placing Professor Dana's two readings side by side, we have,

Sulphur	4.36	1.42

The only remaining point I will notice is contained in the following remark of Professor Dana: —

"In the analysis of corals given in Professor Horsford's paper, no mention is made of the presence of fluorine or phosphoric acid. This will not be received as any reason for discrediting Professor Silliman's determination of these ingredients."

In the first place, I have analyzed no corals. In the second place, the analyses of the crust-rock had only for their purpose to ascertain the ingredients that could take part in the consolidation of the rock. To illustrate the point, I cite an analysis employed by Professor Dana: †

Carbonate of lime	61.93
Carbonate of magnesia	38.07

with the remark of the author, "The magnesia in this analysis was directly determined, the lime being inferred from the loss." How easy to exclaim at such an analysis, in which one ingredient only is determined, and in which certain others, both as to kind and quantity, are inferred, to say nothing of the fluorine and phosphoric acid that

* Three percentages are recorded, while but two determinations are given. The proof-sheets failed to reach me, and I cannot account for this discrepancy, or for another which did not escape Mr. Dana's notice. The average is but slightly varied, and the point in question uninfluenced.

† American Journal of Science, [2] XIV. 82.

may have been present, and which are inferred to be absent! But the case seems different when we understand that the analyst had only the object of determining whether and how much magnesia was present. If this be an analysis of corals, so is mine above.

The following is a summary of the points in which the opinions I have expressed are controverted by Professor Dana.

I. That carbonate of lime held in solution by carbonated water, upon coming under the influence of a higher temperature and diminished pressure, is precipitated.

Professor Dana. "*The common deposition of carbonate of lime from the waters dripping through the roofs of caverns is evidence to the contrary.*"

The reply to this may be found in elementary works on chemistry.

II. That the exceeding fineness of the coral mud is due in part to stone plants (calcareous vegetation) which flourish upon the reefs, and which, admitting of ready reduction to extremely fine powder, contribute to the pulverulent matter resulting from the collision of the coral fragments.

After quoting the above remark, with the omission of the words "*in part*," Professor Dana remarks:—

"*The fineness is due to the trituration of coral fragments and shells where the motion of the water is gentle*,"—"*to the gentle undulations or ripplings of inland waters*,"—"*while the rude sea-shore waves give rise to sand and pebbles.*"

As an application of the theory that the less the agitation the finer will be the powder, the writer asks what degree of fineness would be attained where water is entirely at rest. The writer does not ascribe *all* the fine coral mud to the trituration of the stone plants; a part of it is doubtless derived from corals. Its separation from the coarser grains is due to the currents, which carry the finer particles farther, and deposit them apart from the heavier and coarser grains.

III. That the crust-rock is due to a series of changes, in which the decay of the organic matter, mixed with carbonate of lime, takes part, resulting in the formation of a kind of cement.

Professor Dana. "*A smooth solid crust, two or three lines thick, and made of layers like stalagmite, was formed by the solution of*

lime from the surface by the rains, and its deposition again on evaporation."

Granting, for the sake of argument, this to be true, in instances where the crust-rock covers level areas of acres in extent, after the formation of the lowest layer, whence would material for the succeeding layers be derived?

IV. That the coarse-grained rock owes its solidification, in part, to the fine coral mud and mucilaginous matter which, more or less, filling up its pores, act like cement, and to slow oxidation of the organic matter, giving greater or less solubility to pulverulent carbonate of lime, and permitting it to assume crystalline form.

Professor Dana. "*The solidification is due to the deposition of carbonate of lime from its solution in sea-water, of which it is conceived to be a constant and adequate ingredient.*"

Bibra failed to find carbonate of lime in ten samples of sea-water, submitted to him for analysis. It may be doubted whether it is a constant ingredient in the sea-water about coral reefs, except as derived from the reefs themselves, and it is not obvious that the solubility of carbonate of lime, taken up from coral sand, should lessen on coming in contact with coral sand at a greater depth. What new agency can come into play to precipitate, that was not in action at the time of its solution, Professor Dana does not inform us.

V. A mixture of organic matter, composed partly of vegetable and partly of animal matter, such as is found in the inland depressions near the Florida reefs, and which is regarded as the type of the crust-rock in its earlier stages, and which contains carbon, hydrogen, nitrogen, oxygen, and sulphur, in fermentation and decay, becomes first acid in its reaction, and afterwards alkaline; and as hydrosulphuric acid is volatile, it would be more or less free in the presence of other volatile acids.

Professor Dana. "*Organic matter, consisting of carbon, hydrogen, oxygen, nitrogen, and sulphur, cannot evolve in decay free sulphuretted hydrogen, but hydrosulphuret of ammonia instead; nor can such a mass evolve at any time uncombined ammonium.*"

That the first stages of fermentation and decay are, as a general thing, acid, and that at a period sooner or later, according to the proportion of nitrogenous matter in the organic mass, the alkaline reaction sets in, are settled principles of chemistry. That a volatile acid

like hydrosulphuric acid should, under such circumstances, be free, is as obvious as that carbonic acid should be evolved, — too obvious to require discussion. That ammonia should have occurred in excess seems to me the simplest explanation of the facts observed, to wit, the alkaline reaction, the formation of a film of carbonate of lime at the surface of the water, with which the mud rock was agitated, and the occurrence of a basic carbonate in the rock.

VI. It is suggested, that inasmuch as carbonic acid is not a constant ingredient in sea-water, that since from living corals there is constantly exhaling carbonate of ammonia, that since sea-water contains a large percentage of sulphate of lime, and that since carbonate of ammonia and sulphate of lime mutually decompose each other, there is thus furnished a source of carbonate of lime for corals.

Professor Dana *thinks this may be one source of carbonate of lime* in the sea, *but remarks* "*that the excretions cannot turn about and aid in the secretory processes.*"

I am not unwilling that the suggestion as to the source of the carbonate of lime in corals should stand just as I have given it.

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2. Warwickite a Borotantalite. By Professor J. Lawrence Smith, of Louisville, Ky.

This mineral has as yet been found in no other locality than near Edenville, Orange County, New York. It has been known for some time, and was first described as a distinct species, under the name of Warwickite, by Professor C. U. Shepard, in 1838, and considered by him as a difluorid of titanium and iron, with a trace of yttria; in 1847 it was described again by Mr. T. S. Hunt, under the name of Enceladite, as composed of a hydrated silicate of magnesia, plus the oxides of titanium, iron, and aluminum, $2\,(Fe^2\,O^3 \,.\, Ti^{\overline{2}}\,O^3 \,.\, Al^{\overline{2}}\,O^3) + Mg\,O \,.\, 3\,Si\,O^3 + 2\,HO$. Still later, the same chemist re-analyzed it, only, however, in a very small quantity, the result of which induced him to consider it essentially a titanate of magnesia. This same mineral procured from Mr. Horton, of Craigsville, has been the subject of some recent investigations, and the highly interesting fact discovered

of its containing a large amount of boracic acid, associated with titanic acid, magnesia, iron, and other ingredients not yet thoroughly defined.

It is the first borotitanate, natural or artificial, that is known, and as such of much interest to the chemist as well as mineralogist. Its complex nature and difficulty of analysis have prevented me from obtaining, as yet, exact quantitative results, and although those obtained are doubtless near the truth, it is considered more prudent to reserve them until confirmatory results are obtained. The amount of boracic acid is as much as twenty per cent of the mineral, and the smallest amount of it suffices to give the most marked evidence of the presence of this acid.

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3. Danburite a Silico-Borate of Lime. By Professor J. Lawrence Smith, of Louisville, Ky.

This interesting mineral, as yet only found in this country near Danbury, Connecticut, was first described by Professor C. U. Shepard, as a hydrated silicate of lime and potash, containing a little silicate of alumina and yttria. It was subsequently analyzed by Dr. Erni, and pronounced a silico-borate of lime and the alkalies; containing

Silica	49.74
Lime	22.80
Magnesia	1.98
Soda	9.82
Potash	4.31
Oxide of Iron and Alumina	2.11
Boracic Acid	9.24

In the re-examination of American minerals now making by Mr. Brush and myself, this substance has been under investigation, and the results show it to be a more interesting mineral, and simpler in composition, than had been originally supposed. The results of two analyses are as follows: —

	1.	2.
Silica	48.10	48.20
Alumina and Oxide of Iron	.30	1.02
Manganese	.56	
Lime	22.41	22.33
Magnesia	.40	undeterm.
Boracic acid	27.73	27.55
Ignition	.50	.50

This furnishes an oxygen ratio of silica 4, boracic acid 3, lime 1, or

	Atoms.	Per cent.
Silica	4	49.42
Boracic acid	3	28.02
Lime	3	22.56

$$2\,(Ca\,O\,.\,2\,Si\,O^3) + Ca\,O\,.\,3\,B\,O^3.$$

On comparing the danburite with the only other known borosilicate, datholite, it will be seen that the latter contains exactly double the amount of lime that the former does, the silica and boracic acid remaining the same.

	Silica.	Boracic Acid.	Lime.
Datholite . . .	4 atoms.	3 atoms.	6 atoms, with water.
Danburite . . .	4	3	3

In fact, the formula of the two may be written as follows: —

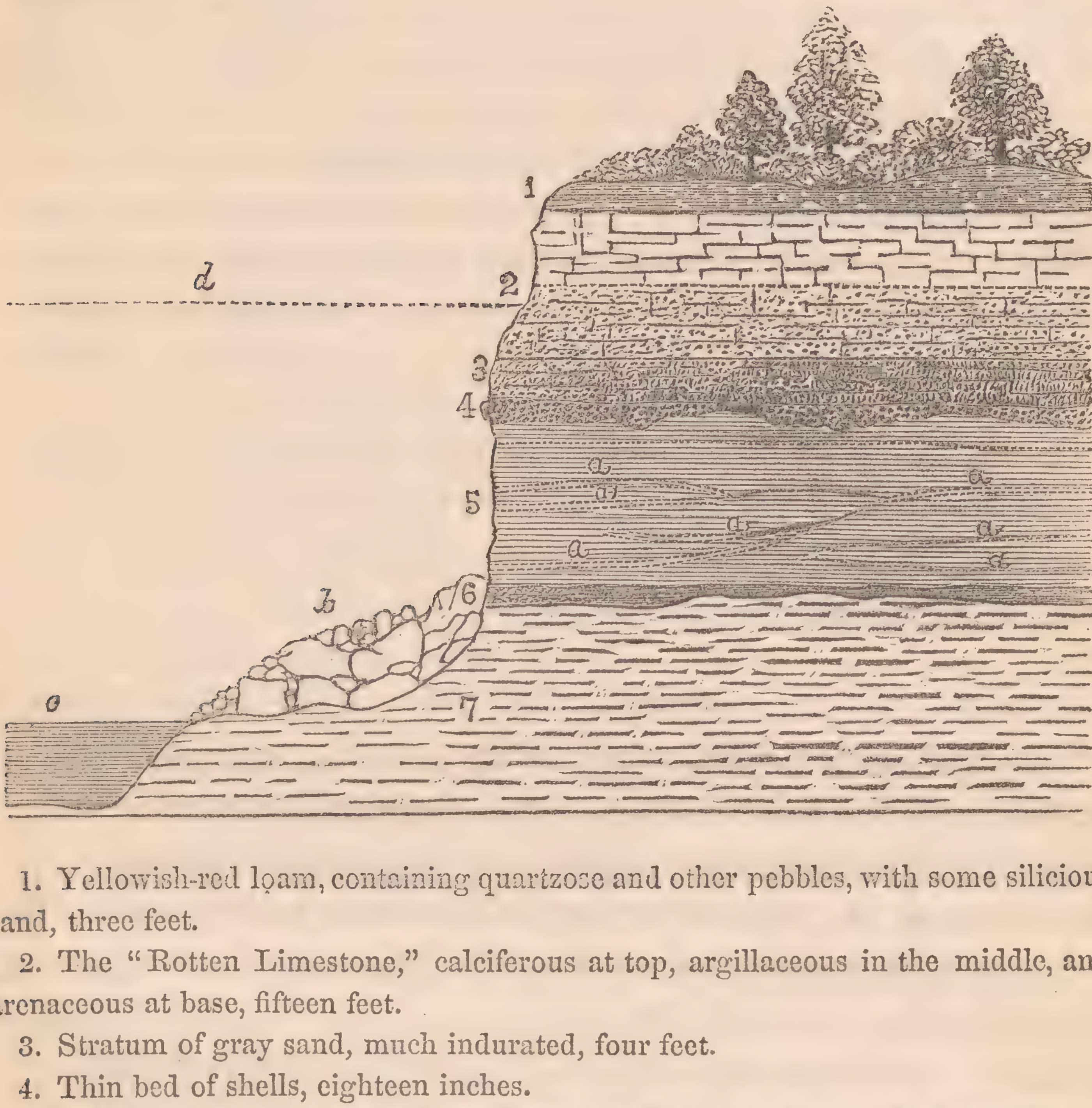
Datholite, $6\,Ca\,O\,.\,8\,Si\,O^3 + 6\,Ca\,O\,.\,6\,BO^3 + 6\,H.$

Danburite, $3\,Ca\,O\,.\,8\,Si\,O^3 + 3\,Ca\,O\,.\,6\,BO^3.$

Owing to the simplicity of the composition of danburite, there was no difficulty in arriving at the amount of boracic acid with perfect accuracy. The danburite forms the second borosilicate of lime.

II. GEOLOGY AND PALEONTOLOGY.

1. On the Geology of the Choctaw Bluff. By A. Winchell, of Eutaw, Alabama.



1. Yellowish-red loam, containing quartzose and other pebbles, with some silicious sand, three feet.

2. The "Rotten Limestone," calciferous at top, argillaceous in the middle, and arenaceous at base, fifteen feet.

3. Stratum of gray sand, much indurated, four feet.

4. Thin bed of shells, eighteen inches.

5. Bed of yellowish-gray sand, very compact, obliquely laminated, fifteen feet.

6. A bed of green sand, one foot.

7. Dark-colored shale, sandy at top, and constituting the bed of the river, five feet to surface of low water.

b. *Débris* from the bluff.

c. Surface of the river at low water.

d. Surface of the river at high water.

Choctaw Bluff is on the west bank of the Black Warrior River, six miles south of Eutaw, in Greene County, Alabama. It is nearly half a mile long, and elevated about sixty-five feet above low water. The land immediately adjacent is a patch of "post-oak" soil, situated as

usual in the "black lands" or "prairies." The opposite bank of the river and the country beyond is, as in every other case, low and alluvial.

2. The stratum of limestone at this place, and, as far as I have observed, throughout the formation, is considerably decomposed at top, and discolored probably by the infiltration of ferruginous solutions from the overlying loam. At the depth of two or three feet the rock becomes harder, and below this passes gradually into a marly shale, with a large admixture of sand. The lower fourth of this stratum (2) is exceedingly arenaceous, with comparatively little admixture of lime or clay. The upper and lower portions therefore differ widely from each other and from the intermediate portion, but it is impossible to settle upon any line marking the termination of one portion and the beginning of another. This stratum is quite cleavable and fissile, and abounds throughout with nodules of bisulphuret of iron. The rock is so soft as to be cut easily with a knife. The more calcareous portions when dried have a white appearance, remotely resembling chalk. These strata contain *Ostrea plumosa*, *Hamites onyx*, *Inoceramus Barabini*, and another species of gigantic dimensions, which I have not seen described. Some *sharks' teeth* are also found, and at the base *Mosasauroid* remains occur; but, on the whole, the limestone of this bluff is not rich in fossil remains. This is the stratum termed "arenaceous" by Morton, who considers it as belonging to the older cretaceous rocks. In point of fact this is the newer cretaceous stratum of Alabama, while the newer cretaceous of Morton is now well known to belong to the newer Eocene.

3. This stratum of gray sand is very much indurated, and of very uniform thickness and composition. In wearing away, the fragments do not disintegrate, but lie in large blocks at the base of the bluff. They abound in *Exogyra costata*, *Ostrea cretacea*, *O. panda*, *Pecten quinquecostatus*, *Inoceramus* (species unknown), *I. Barabini*, *Teredo tibialis*; imperfect casts of *Ammonites Delawarensis*, casts of *Hamites torquatus*, *H. arculus*, *Turritella vertebroides*, and species of *Voluta*, *Buccinum*, and *Cardium*; teeth of *Psammodus*, *Ptychodus Mortoni* and *P. polygyrus*; sharks' teeth of the genera *Otodus*, *Oxyrhina*, *Corax*, and *Lamma*; vertebræ of cartilaginous fishes, remains of *Corals*, and one or two species undetermined, and perhaps new. The lower portion is richest in remains. One specimen of the species of *Inoceramus*

referred to above (2) measured three feet in length, the valves being also in horizontal juxtaposition. Another specimen was eighteen inches high and five inches thick. These remains generally lie in a horizontal position, and the proximity of the valves is common both in this species and *Exogyra costata*. This stratum contains some fossil wood.

4. A remarkable bed of shells, being almost entirely composed of *Ostracea*, the most abundant of which are *Exogyra costata* and a species of *Gryphæa*, which seems to be intermediate between *G. convexa* and *G. mutabilis*, some specimens approaching the one form and others the other. The shells of this bed are for the most part firmly cemented, forming a stratum of difficult disintegration, and thus being the most projecting portion of the bluff. This shell bed is remarkable also for being flat on its upper surface, and irregular below.

5. Sand, very compact, but not cemented. The first four feet are gray; the remainder is generally greenish or grayish-yellow. The bed is very distinctly marked by oblique lamination, having the exact appearance of sand washed up and deposited upon a beach. Thin unconformable lamina of dark-green sand (*a*, *a*, &c.) pass in horizontal and curved planes through the main bed. Few animal remains have been noticed in this bed, but plenty of fossil drift-wood is seen cropping out, which is often completely riddled with *Teredo tibialis*.

6. Green sand of broken continuity and varying thickness. In many places it contains long, narrow strips of fossil wood, black, compact, and carbonaceous, much resembling lignite. This bed is here (and everywhere) water-bearing.

7. Shale, of a dark color, and near the top quite sandy, containing no remains, but abundant nodules of iron pyrites. This stratum constitutes the bed of the river at this place. Below this, shale and green (sometimes gray) sand alternate with each other to the depth of several hundred feet. They do not seem to be disposed in strata of any general extent, the succession of layers in no two places being the same, if we except the occurrence of the two beds of sand marked 5 and 6, and a lower bed about a hundred feet deeper. Besides these, the sand is limited to frequent oblique laminæ of small extent, interposed between layers of shale. This is ascertained by the deep borings of Artesian wells, which derive all their water from the upper (6)

and lower beds. It is also observed along the outcrop of the strata, which can be traced northward to within a few miles of Tuscaloosa, along the Warrior, and to the Centreville Station, in Bibb County, on the Selma and Tennessee River Railroad.

As a general statement, the fossils of this bluff are not very well preserved. The *Ostracea*, though abundant, are much water-worn and fractured. *Ostrea cretacea* is an exception. *Pecten* is seen entire only as it is firmly imbedded in the sand. The remains of fishes are in a brilliant state of preservation, and abundant.

Notwithstanding the imperfect condition of so many of the fossils, this bluff is uncommonly interesting for its great variety of species, and for exhibiting a section of all the important rocks of the series, being thus an epitome of the entire formation. Prairie Bluff, one of the classical localities of Alabama, furnishes a few species peculiar to itself, many abundantly, and all in a very good state of preservation; but Choctaw Bluff is worthy of particular study, as affording in itself a very good knowledge of the paleontology and stratigraphical relations of the entire cretaceous group of the State.

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2. On the Parallelism of the Lower Silurian Groups of Middle Tennessee with those of New York. By Professor James M. Safford, of Lebanon, Tennessee.

The Lower Silurian rocks of Middle Tennessee are divided into two natural and well-characterized groups. The lower division, which has been named the Stone's River Group, is a series of bluish and dove-colored limestones from two hundred and fifty to three hundred feet thick. These rocks are the lowest visible in this part of the State.

The upper division, named the Nashville Group, is, in great part, dark-bluish limestone about four hundred feet in thickness. We are acquainted with two hundred species from these rocks, of which one half are new, the others being identical with New York species.

The Tennessee strata under consideration are the equivalents, generally, of the following New York groups: first, the Black River

group (including the Chazy, Bird's-eye, and Black River limestones); secondly, the Trenton limestone; and thirdly, the Hudson River group (including the Utica slate).

This general parallelism is very clear and satisfactory. When we come, however, to search, in Tennessee, for the Trenton limestones, separated, as a distinct group, from the Black River rocks below, and from the Hudson River above, we are entirely lost.

The difficulties are these. First, many of the species belonging exclusively to the Trenton limestone in New York occur, in Middle Tennessee, mingled in the same strata with Black River fossils; in fact, many of them occur in a lower position than some of the Black River species; for instance, the following group, *Stromatocerium rugosum*, *Streptelasma profunda*, and *Columnaria alveolata*, is highly characteristic of the uppermost member of the Stone's River group, notwithstanding the Trenton part of the same group affords such Trenton fossils as the following: *Retepora fenestrata*, *Subulites elongata*, *Cyrtolites compressus*, *Bucania bidorsata*, *Bucania expansa*, &c., &c. In the second place, if we take the Nashville group, and study its Trenton and Hudson River fossils, we find the same blending of species, some of the Trenton running up to the very top of the group, and some of the Hudson River appearing at its base. The Trenton species thus appear to lose their value, in Middle Tennessee, as characteristic of a subdivision of the Lower Silurian rocks.

It is very different, however, with the species of the other New York groups. The Stone's River group has throughout (excluding the Trenton species) a well-marked Black River fauna; and so the Nashville group, which succeeds it, has a decided Hudson River fauna, while at the same time there is no blending of these characteristic species.

To illustrate these remarks, I have constructed the following table, using all the described species common to the two States, excepting those found either in both the Tennessee groups, or in both the Black River and Hudson River groups of New York, for these do not bear upon the points before us; this excludes such species as *Orthis testudinaria*, *Pleurotomaria umbilicata*, *Leptæna sericea*, *L. alternata*, *Chætetes columnaris*, *Murchisonia bicincta*, &c., &c. Several *doubtful* species have also been excluded.

	New York.						Tennessee. Stone's River Group.			Tennessee. Nashville Group.		
	Chazy.	B. & B. R.	Low Tren	Cen. Tren	Up. Tren	U. & H.R.	Low	Cen.	Up.	Low	Cen.	Up.
1. Maclurea magna	*	*						*				
2. Columnaria alveolata		*							*			
3. Streptelasma profunda		*							*			
4. Stromatocerium rugosum		*					*		*			
5. Gonioceras anceps		*					*	*				
6. Litulites undatus		*						*				
7. Orthoceras fusiforme		*					*	*	*			
8. Buthotrephis ? cæspitosa			*					*				
9. Retepora fenestrata			*					*				
10. Leptæna filitexta			*					*	*			
11. Orthis tricenaria			*					*				
12. Bucania bidorsata			*					*				
13. Bucania expansa			*					*				
14. Pleurotomaria rotuloides			*						*			
15. Cyrtolites compressus			*		*			*				
16. Phacops callicephalus			*		*			*				
17. Edmondia ventricosa				*	*			*				
18. Ambonychia amygdalina					*			*				
19. Endoceras proteiforme				*	*			*				
20. Subulites elongata					*			*				
21. Spirifer lynx			*	*	*					*	*	*
22. Orthis pectinella			*	*	*						*	
23. Murchisonia bellacincta			*		*						*	
24. M. suffusiformis					*						*	
25. Atrypa modesta					*						*	*
26. Modiolopsis anodontoides										*		
27. Favistella stellata											*	*
28. Ambonychia radiata											*	*
29. Avicula demissa											*	
30. Cyrtolites ornatus											*	

This table illustrates the blending, in Tennessee, of Trenton with Black River species below, and Hudson River species above; and also the fact, that the characteristic Black River species are confined to the Stone's River group, while those characteristic of the Hudson River rocks are confined to the Nashville group.

In view of all these facts, it follows, first, that the Trenton lime-

stone, as a distinct group, cannot be recognized in Middle Tennessee; and, secondly, that the Nashville and Stone's River groups are respectively the representatives of the Hudson River and Black River groups of New York, and that the former rested directly upon the latter.

It may be added, too, that the facts thus developed in Tennessee show that it will hardly be satisfactory to unite, as has been suggested, the Trenton limestone, as a group, with the Hudson River rocks, for the blending of species takes place downwards as well as upwards, in as great, if not greater, proportion. So far as our Tennessee species are concerned, it would be a much more natural arrangement to unite the lower part of the Trenton limestone with the Black River rocks, and the central and upper portions with the Hudson River group. The table, which has been constructed with reference to this view, illustrates this point sufficiently well. Most of the Trenton species in the Stone's River group belong to the lower division, and the few others may be found hereafter to belong to the same; while all of the Trenton species in the Nashville group belong to the upper division, three of them being common.

If the New York species will admit of this classification, and we are inclined to think they will, the confusion which has hitherto existed in regard to the parallelism of these groups will in some measure, at least, be removed.

The parallelism will then be

Nashville group.	Hudson River. Utica Slate. Cen. and Upper Trenton.	*New group.*
Stone's River group.	Lower Trenton. Black River group.	*New group, say* Black River.

3. On the Structure and Affinities of Certain Fossil Plants of the Carboniferous Era. By Dr. J. S. Newberry, of Cleveland.

Dr. Newberry remarked, that it had been anticipated that, on an examination of the fossils of our wide-spread American coal-fields, specimens would be found which, by the perfection of their preservation, would throw new light on the structure and affinities of many fossil plants in regard to which these points had been enveloped in doubt; as well as new forms be discovered which would aid in forming an intelligent idea of the flora of the Carboniferous era; and that the fossil botanists of Europe were looking to the results of our investigations with no little interest.

In accordance with this anticipation, even a brief and limited examination of the plants of the Ohio coal-field had furnished him with specimens which serve to illustrate better than has before been done the structure and affinities of several genera and species of coal-plants; among others, he would call the attention of the Section to his specimens of

Trigonocarpon,

which he said was a fossil fruit not unfrequently found in the sandstones of the coal series, usually presenting the appearance of an

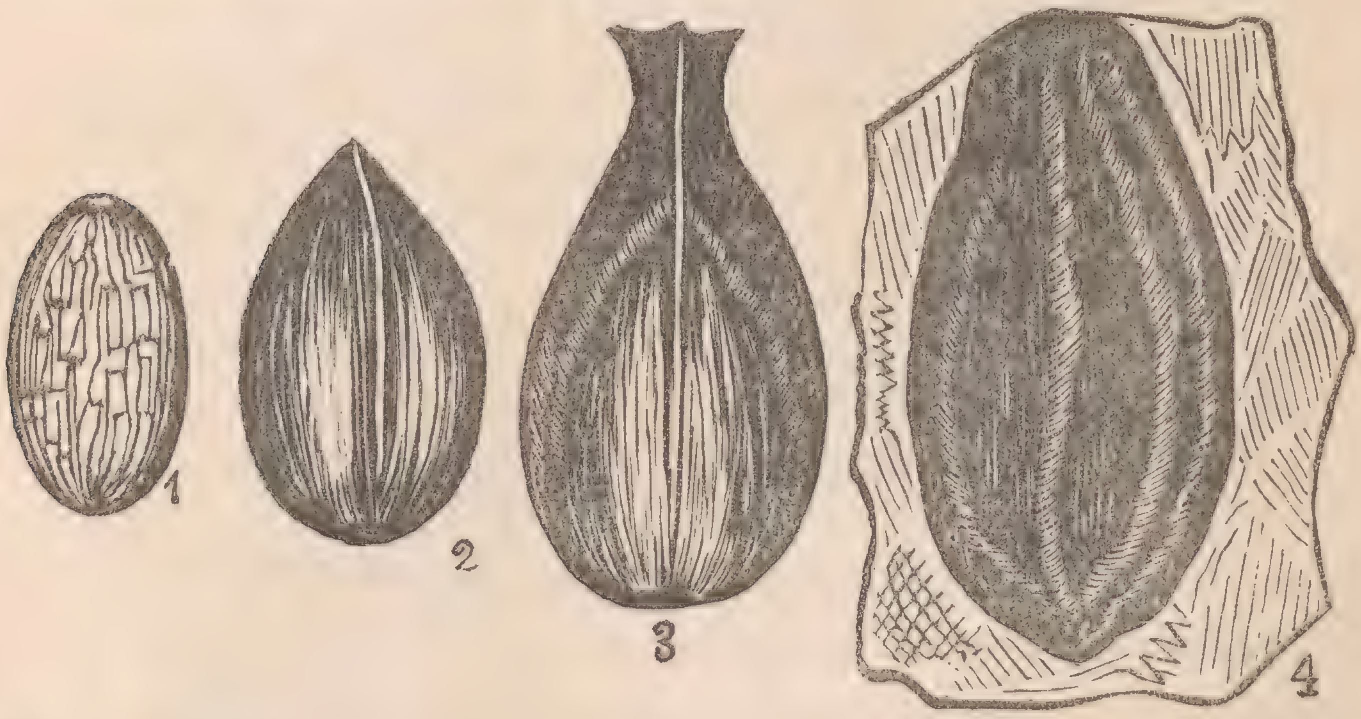

Fig. 1. Nucleus.
Fig. 2. Nut as usually found.
Fig. 3. Nut, perfect form.
Fig. 4. Drupaceous envelope containing nut, fossilized in shale.

ovoid pointed nut marked with three elevated lines, without any traces of internal structure or external appendages. He had been so fortunate as to obtain specimens, in which the fruit was shown to be composed of an ovoid *nucleus* covered with a kind of reticulated marking, not unlike a nutmeg. Exterior to this was the *shell*, the only part visible in most specimens, which was divided into three segments, the segments separating at the ridges, which were, when perfect, prolonged into wings, which wings united at the apex to form a triangular column, expanding again to form a small triangular area. This was the perfect *nut*, outside of which was a fleshy drupaceous envelope, which led him to compare the entire fruit with a date.

This structure closely resembles that of the fruits of some species of Palms and *Cycadeæ*.

He was disposed to regard *Trigonocarpon* as the fruit of an extinct family of plants, allied to both Palms and *Cycadeæ*, probably distinct from both, and not, with Unger, as belonging to the *Scitamineæ*, nor, with Hooker, as the sporangium of a *Lepidostrobus*, nor yet, with Lindley, as unquestionably the fruit of a Palm.

Cyclopteris.

The difficulty of separating *Cyclopteris* from *Neuropteris* had led both Brongniart and Goeppert to suggest that, in many species at least, the folioles from different parts of the plant varied so much in form, that they had been included in different genera.

Dr. N. said that his observations had led him to the same result, and that he had seen several species of *Neuropteris*, and one at least of *Odontopteris*, of which a certain number of pinnules were of the strangest and most anomalous forms, either rounded to form *Cyclopteris*, or deeply cut and lobed, palmate, &c., presenting as great a variety of form as in the recent *Allosorus sagittatus* or *Lindsaea cordata*.

Asterophyllites and Sphenophyllum.

The jointed striated stem, the verticillate arrangement of the leaves, and similar fructification, have been considered as proofs of the close affinity of these two genera, but the wedge-shaped leaves of *Sphenophyllum* have been regarded as quite distinctive. Dr. N. said, however, that many species of these two genera should be united, being different portions of the same plant; *Asterophyllites* being the

lower part, with numerous capillary or linear leaves, while *Sphenophyllum* was the upper part, with broad wedge-shaped leaves.

Fig. 5.

Fig. 6.

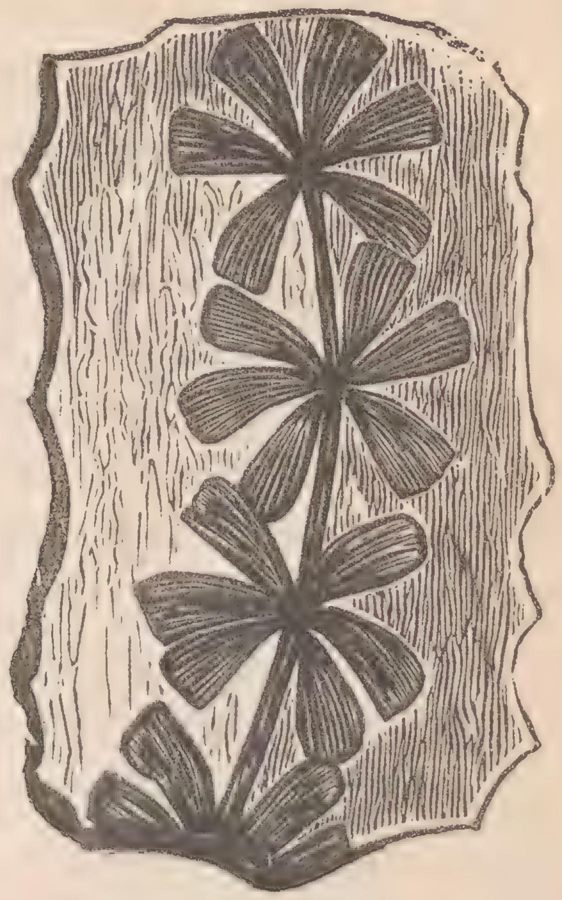

Fig. 5. Cauline leaves, — *Asterophyllites*.
Fig. 6. Terminal leaves, — *Sphenophyllum*.

The difference of form between the superior and inferior leaves of *Sphenophyllum* would seem to indicate that it was an aquatic plant, the submerged leaves being linear, uni-nerved, almost capillary, and the emerged leaves broad, with a compound nervation, precisely as in many recent aquatic plants. This supposition is further strengthened by the extreme length and tenuity of the branches of this apparently herbaceous plant, which would seem to have required the support of a denser medium than air, and by the discoid expansion of the leaves in each verticil as generally preserved on the shale; an arrangement scarcely possible, unless, as in *Annularia*, the verticils of leaves were expanded on the surface of the water. The spikes of the fructification of *Asterophyllites* and *Sphenophyllum* (*Volkmannia*, Sternb., Presl. *Asterophyllites tuberculata*, Lind. & Hut. Tab. 108) are not uncommon in the coal-mines of Northern Ohio, where they sometimes exhibit the superior and inferior terminations, and with considerable distinctness the details of the internal structure.

Lepidostrobus.

Dr. N. exhibited specimens of a cone of Lepidodendron which was

pedunculate, resembling very closely the fructifications of some recent *Lycopodia.* The internal structure was, however, different from that of any recent species, there being six or more globules in the axil of each of the scales.

ANTHOLITHES.

Dr. N. exhibited to the Section a beautiful spike of fossil flowers from the carboniferous strata, saying that this was the first instance of the discovery of distinct *flowers* in the coal strata; the so-called *Antholithes* of Lindley being probably not a flower, but a fruit, and that a *Cardiocarpon.* He continued thus: —

Fig. 7.
Antholithes priscus, NEWBERRY.

"This beautiful fossil affords us, perhaps, the first unquestionable evidence of the existence of plants with conspicuous flowers at the period of the deposition of the carboniferous strata. The abundance of fossilized coniferous wood in the coal-bearing rocks, proves the existence of gymnospermous phænogamous plants at that early era, — and perhaps the plant of which this was the florescence had no higher organization; but its graceful spike will be looked upon with peculiar interest, as proving that the gloom of the sombre forests of the coal period was enlivened by the same objects that most adorn the field and forest of the present day.

"And although the only air-breathing animals of which the remains have been yet found in the carboniferous strata are lizards, scorpions, and cockroaches, we may be sure there were other eyes than theirs to mark its beauty of coloring and grace of form; at least, it looks so much like our now-a-day flowers, that we cannot refrain from conjecturing that its petals were often brushed by the wings of the wandering bee, as he came, attracted by its fragrance, to sip the honey it distilled.

"When we take into consideration the unusual circumstances which must attend the fossilization, in anything like a perfect form, of any of the flowers of the present day, we need not be surprised that they are rarely found preserved in the rocks of the different geological epochs; and we have no reason to infer, from the scarcity of the remains of objects so delicate and perishable, that the flora of the ancient world was wholly barren and beautiless.

"Although preserved with great distinctness and beauty, it is quite impossible to determine with certainty the number or character of the floral envelopes in this unique fossil, or its precise place in the vegetable kingdom. At first sight it resembles one of the *Compositæ* with an imbricated involucre and a few ray florets, and such is especially the appearance of the flower when compressed so as to present a discoid expansion of its parts; but this resemblance is so vague and general, that it affords no very strong ground for supposing that the order of the *Compositæ* dated from so remote an epoch. It resembles, perhaps, as much some of the *Yuccas* or *Bromelias*, and in their vicinity I should be disposed to place it.

Fig. 8.

Fig. 9.

Fig. 8 represents a portion of a spike of seed-vessels found associated with Fig. 7, and which may possibly have been the female flower of the same plant. The detached capsules would be included in the genus *Cardiocarpon.*

Fig. 9. A different species of Fig. 8, or the same at a different stage of maturity.

"I have found this fossil only in one locality, in Mahoning County, associated with Figs. 8, 9, and enveloped in great quantities of the

leaves and stems of *Noeggerathia*. So intimate and exclusive is the companionship of these fossils, that I have been led to suspect them to be but parts of the same plant. If this were true, we should have a stem resembling, in its external markings, the stems of the *Yuccas*, but with a different internal structure; leaves having somewhat the form, and almost precisely the nervation, of the same family, to which would belong appropriate flowers; an *ensemble* which would be closely allied to the *Liliaceæ* among living plants, and yet separated from this order by the anatomical structure of the stem."

All these fossils are found associated with the lowest stratum of coal in the coal-basin of Ohio.

Artisia.

This genus was founded on the casts of the medullary centres of fossil plants of the genus *Lepidofloyos*. The same structure had been found in *Flabellaria borassifolia*, and he had found it also in the stems of *Noeggerathia*.

Cannophyllites.

This genus, supposed to be a representative of the family of *Canneæ*, Dr. N. said was a fern; of the entire frond of which he exhibited a specimen.

Dr. N. further said, that much surprise had been expressed at the rarity of the trunks of fossil *tree-ferns*, as it had been supposed that the immense fern fronds, six to eight feet in length, which are found in the coal-mines, must have belonged to *tree-ferns;* and while these fronds were very common, specimens of the genus *Caulopteris* (fossil tree-ferns) were so rare, that he had never found one in Ohio. He was convinced that this difficulty could be explained by the supposition that these huge fronds sprang from a subterranean rhizoma, and not from an upright trunk. He had specimens of several large fronds, which must have resembled, when growing, our common *Pteris aquilina*.

Professor Hall remarked that the paper of Dr. Newberry was of great interest and value, as indicating a more complex and perfect flora, during the coal period, than has been supposed to have existed. The *fauna* of the same period had been shown by recent discoveries to have been more perfect than was formerly supposed.

4. On the Carboniferous Flora of Ohio, with Descriptions of Fifty New Species of Fossil Plants. By Dr. J. S. Newberry, of Cleveland.

Dr. Newberry said he had hoped to be able to present to this meeting the results of a series of observations on the geographical and stratigraphical distribution of fossil plants, comparing the fossil floras of different localities in this country with each other, and with that of Europe; but that a protracted illness had prevented him from visiting localities and collecting facts such as he had hoped to do. He had, however, made a comparison of the coal-plants of Pennsylvania and such as had already been collected in Ohio, and had been surprised to find so great a discrepancy. Of the species collected, scarcely one in ten were common to the two States. This difference was due, partly, to the geographical distribution of species, which in the former epochs, as now, gave to different districts somewhat different floras, but more to the changes which had been effected in the flora of the same locality during the deposition of the different carboniferous strata, giving to the upper beds of coal a very different catalogue of plants from that of the lower ones. In Pennsylvania the proximity of igneous rocks had nearly obliterated the vegetable impressions from the roof stone of the lower beds; consequently, most of the specimens from that State had been collected from the upper beds, while in Ohio the largest collection had been made from the lowest beds in the series, where the flora was much the richest.

The shales and sandstones associated with the lowest stratum of coal in Northern Ohio furnish a greater variety of fossil plants than have, perhaps, ever been found elsewhere, within equal geographical and geological limits.

In Europe, rarely more than thirty or forty species accompany a single coal stratum, the lower beds in a basin being least rich in fossil plants; the number of species associated with each sometimes not exceeding eight or ten.

It is but a few years since the first coal-mine was opened in this region, and up to the present time so little of the field has been thoroughly explored, that it is quite too early to attempt to estimate the number of species which are here fossilized. He had, however,

already formed a catalogue of about one hundred and fifty distinct species, and had imperfect specimens of many more; all collected from about half a dozen localities within a few miles of each other, and within a vertical range of less than a hundred feet, including but a single coal-seam. From the number of species already collected, taken in connection with the fact, that the plants of a former world had somewhat the local habitat of those of the present day, — each coal-mine having a florula in some respects peculiar to itself, and each new mine that is opened furnishing new species, — it is evident that the fossil flora of this limited district is remarkably rich.

The fossil plants of this region are distributed among forty-one genera, as follows: —

Calamites	10	Diplazites	1
Calamodendron	3	Hymenophyllites	1
Artisia	2	Adiantites	2
Sigillaria	15	Coniopteris	2
Syringodendron	3	Schizopteris	2
Lepidodendron	8	Cannophyllites	1
Lepidostrobus	3	Noeggerathia	2
Lepidophyllum	3	Pychnophyllum	1
Lepidofloyos	3	Casea	1
Megaphytum	1	Whittleseya	1
Knorria	3	Pinnularia	1
Bornia	1	Polysporia	1
Halonia	1	Cardiocarpon	8
Stigmaria	2	Trigonocarpon	3
Hydatica	1	Rhabdocarpus	2
Lycopodites	1	Carpolithes	5
Sphenopteris	24	Asterophyllites	3
Neuropteris	4	Annularia	1
Pecopteris	9	Sphenophyllum	4
Alethopteris	3	Antholithes	2
Odontopteris	2	Peuce	1

Dr. Newberry said this catalogue gave a very good generic picture of the flora of the base of the productive coal measures, at least in Ohio: it was characterized by a large number of species of *Sphenopteris*, *Calamites*, *Sigillaria*, and *Lycopodiaceæ;* that it contained

representatives of nearly all the genera of the carboniferous period, and a large proportion of the most highly organized plants of that era.

From the comparison, so far as made, between the fossil plants of the lower and upper parts of the productive coal measures in Ohio, it would seem that there had been a gradual change in the vegetation of the same locality, both generic and specific; the *species* of the older beds, almost without an exception, being succeeded by others, while the generic changes were confined to the extinction of a small number, with the introduction of others.

Brongniart noticed in the coal-fields of France that *Calamites* and *Lepidodendron* were most abundant in the lower beds; *Sigillaria*, in the middle and upper beds; *Asterophyllites*, and especially *Annularia*, in the upper coal strata.

Dr. Newberry had found in Ohio that *Calamites*, *Lepidodendron*, *Sigillaria*, and *Sphenopteris* were most abundant in the lower beds; *Cyclopteris*, *Annularia*, *Asterophyllites*, *Neuropteris*, *Pecopteris*, were most numerous in the middle and upper beds, and *Psaronius* was peculiar to the upper; — that, as far as his observations had extended, the place of *Alethopteris lonchitidis* was at the base of the series; *A. Serlii*, *Neuropteris cordata*, *Pecopteris arborescens*, *P. Cyathea*, *Sphenophyllum Schlotheimi*, *Dictyopteris obliqua*, in the middle and upper beds, &c.

Dr. Newberry said, that, in the comparison of American with European fossil plants, a very large proportion of the species collected here were regarded as identical with those of Europe. He thought the matter required a thorough revision; that when there had been a more careful comparison of well-characterized *specimens*, European with American, it would be found that the number of species common to the two continents had been much overrated; that of recent plants, as well as animals, a large number of species were at one time considered common to the two continents; but more recently, constant characters had been observed in most of these species which served to separate the one from the other. In recent Cryptogamic botany, many species which at first sight seemed identical with those of Europe had been found upon careful study to be specifically distinct. His observation, so far as it had extended, prepared him for the same result following a careful examination of our fossil plants.

He said, that, of the new species of which he had now submitted descriptions to the Association, nearly all were from Northern Ohio, which was due to the fact that this region had been most carefully studied by him, and the specimens from which Brongniart's and Bunbury's descriptions and figures of American fossil plants had been drawn were mostly from the upper part of the series, where, as before stated, the flora was specifically quite different.

Dr. Newberry remarked, in conclusion, that the science of fossil botany was destined to become of the highest importance and value to the geologists, as fossil plants were as reliable criteria for the classification of strata as molluscous fossils ; and an accurate knowledge of the geographical and stratigraphical distribution of fossil plants would throw great light on the former state of our planet.

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5. On the Fossil Fishes of the Cliff Limestone of Ohio. By Dr. J. S. Newberry, of Cleveland.

Dr. Newberry exhibited to the Section a very interesting series of remains of fossil fishes, which he had obtained from the Cliff Limestone of Ohio. He compared this limestone with the fish-beds of the Old Red Sandstone of Scotland, of which he regarded it as the geological equivalent, and the rival in the number and interest of its fossil fishes.

Dr. Newberry said, that of the European Devonian fishes there had already been found — in or near the Cliff Limestone — Asterolepis and Holoptychius ; and he was now able to present to the Association the remains of Coccosteus and Cephalaspis, from the same deposit. He remarked further, that the remains of fishes had been found widely scattered over the West, — wherever, indeed, the Cliff Limestone or its equivalents had been noticed ; that they had been found in Ohio, New York, Indiana, Illinois, and Michigan.

Professor Hall remarked at length on the paper of Dr. Newberry, expressing his interest in the discovery of two additional genera of European fossil fishes in the rocks of this continent ; requesting Dr. Newberry to continue his exploration of this rich field, and make the subject the theme of a special report at the next annual meeting.

Professor Hall then went into a learned, but lucid exposition of the principles which should guide us in the classification of our sedimentary rocks, and of the difficulties which we must necessarily encounter in any attempt to harmonize our geology with the artificial classification of European geologists. This he very strikingly illustrated by an inquiry into the geological age — i. e. whether Carboniferous, Devonian, or Silurian — of the Cliff Limestone. This rock was composed at the West of four widely different, and in New York widely separated deposits, namely, Upper Helderberg, Lower Helderberg, Niagara, and Galena Limestones, of which the upper contained at least two species of carboniferous Brachiopoda, and the lower was distinctly Silurian.

Professor Hall illustrated by a great number of examples the paleontological evidence of the interlocking of the different geological systems, — showing that in former times, as now, physical geography, climate, ocean currents, depths and shallows, had affected the distribution of species, and had produced in each period local and dissimilar faunas.

Professor Hall's generalizations in reference to the relations of sea and land, i. e. to the geography of a former world, were especially interesting.

III. GEOGRAPHY.

1. Recent Discovery of a Deep-Sea Bank on the Eastern Side of the Gulf Stream, off the Coast of South Carolina, Georgia, and Florida, by Lieutenants-Commanding Craven and Maffitt, U. S. N., Assistants Coast Survey. Presented by Professor A. D. Bache, Superintendent.

The Gulf Stream has been explored in connection with the Coast Survey, by running sections perpendicularly across it at different points in its course, and exploring the temperature, and, as far as practicable, other phenomena of the stream, at stations on those sections; and from the surface to depths of six hundred, and in some

cases of twelve hundred fathoms; the stations being selected at greater or less distances, according to the less and more rapid changes occurring in the portion of the stream which they were intended to explore. In the exploration made in June last, the hydrographic party of Lieutenant-Commanding Craven was instructed to examine the stream on four sections, beginning with one from Cape Canaveral, Florida, perpendicular to the direction of the stream; next, taking up one across it from St. Augustine; next, from St. Simon's, Georgia; and last, from Charleston, S. C.

The party of Lieutenant-Commanding Maffitt was to run over the same section from Charleston, and then take up others in succession, farther north. The section from Charleston was explored by Lieutenant-Commanding Maffitt's party between the 2d and the 11th of June, and soundings were kept entirely across the stream, at depths of less than 600 fathoms, the bottom being brought up. The longitude reached was 77° 12′.

On the 11th of June, Lieutenant-Commanding Craven, having crossed the Gulf Stream without finding bottom at 1,000 fathoms, came upon a deep-sea bank at the depth of 469 fathoms, in latitude 28° 24′ N., longitude 79° 5′ W. This bank was again struck on the section north of this one at similar depths, bottom being brought up, and it was traced thus to a position comparing with the Charleston section, where it had been struck by the other party, and bottom brought up from the depth of 300 fathoms, in latitude 31° 37′, and longitude 78° 33′, on the 7th of June.

This bank is supposed to be an extension of the Bahama Banks, and will be carefully explored. Its discovery is claimed for the officers whose names are at the head of this article, and for the vessels with which they are connected.

The following interesting remarks in regard to the nature of the bottom brought up are made by Assistant L. F. Pourtales, who has examined the specimens deposited by Lieutenant-Commanding Craven in the Coast Survey Office, and has compared them with those of the cross-section off Cape Henlopen, formerly examined by him. Mr. Pourtales in a letter to me says: —

"I have in hand now the specimens of bottom from the Gulf Stream, obtained by Lieutenant Craven, and can say they are among the most interesting I have ever seen. You recollect I said in my

report, that with the increase in depth (in the greater depths) the number of individuals appeared to increase. The greatest depth from which I had seen specimens was between 200 and 300 fathoms. There the sand contained perhaps 50 per cent of Foraminiferæ (in bulk). The specimens now before me go to 1,050 fathoms, and there is no longer sand containing Foraminiferæ, but Foraminiferæ containing little or no sand. The grains of sand have to be searched for carefully under the microscope to be noticed at all. The species are the same as found in the deep-sea sounding, in Section 2, but the specimens look fresher and appear somewhat larger. The *Globigerina rubra* of D'Orbigny, which forms the majority, has frequently that delicate pink color to which it owes its name, but which I cannot recollect to have noticed in northern specimens. There are also some species of coral and dead shells from the depth of 1,050 fathoms. The corals do not look much worn, but still appear to have been dead. There are some delicate shells of mollusks from depths beyond 500 fathoms, where they were certainly living."

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2. On the Measurement of Heights by the Barometer. By Professor Elias Loomis, of New York University.

Laplace has given in the *Mécanique Céleste*, Book X., Chapter IV., the formula for the measure of heights by a barometer. This formula is sufficiently easy of application to one who is versed in mathematical computation; but a beginner, in attempting to use it, is quite liable to make mistakes, as it requires multiplication by a logarithm; or if the entire operation is performed by logarithms, it requires us to use the logarithm of a logarithm, an operation which beginners are sometimes slow to comprehend. Numerous attempts have therefore been made to facilitate the computation by developing some of the factors in a tabular form. Among the tables constructed for this object, those of M. Oltmann have been most extensively approved. They dispense entirely with the use of logarithms, and reduce all the factors to a tabular form. The calculation by these tables is readily made, and there is little liability to serious mistake, even in the hands of an or-

dinary arithmetician. The measurements are all expressed in meters, and in degrees of the centigrade thermometer. If these tables are to be applied to the English standards, the readings of the English instruments must first be converted into French measure. This inconvenience has greatly impaired the value of Oltmann's tables for English observers. Attempts have been made to obviate this inconvenience, by reducing all the numbers in Oltmann's tables to English measure; but these translations are inconvenient for use, because the arguments of the French tables, being meters and millimeters, when translated into English, exhibit inconvenient fractions, which render interpolation very awkward and troublesome. Such tables have therefore always appeared to me more inconvenient than to translate the English data into French measure, and then to use the French tables.

In the French *Annuaire* for 1852, M. Mathieu has given new tables similar to those of Oltmann, but more extensive, and adopting the results of more recent determinations. I have been desirous of rendering these tables as useful to the English observer as the originals are to the French observer; but for the reason already stated, this could not be done by the simple translation of meters into feet and inches. I have therefore reduced all the constants of Laplace's formula into English measure, and have thence computed, entirely anew, tables similar in form to those of the French *Annuaire.* These tables, therefore, are not translations from the French, but original computations.

These tables are five in number. Table I. furnishes the approximate height of the upper station in English feet, for a mean temperature and pressure. Table II. gives the correction of the approximate height, depending upon the difference of the temperatures of the two barometers. Table III. gives the correction for the variation of gravity with the latitude. Table IV. gives the correction due to the diminution of gravity on a vertical; and Table V. furnishes the correction due to the height of the lower station.

Method of Computation.

Take from Table I. the two numbers corresponding to the observed barometric heights. From their difference subtract the correction from Table II., which gives the approximate altitude, A. Multiply the nine-hundredth part of A by the sum of the temperatures at the two

stations diminished by 64. Applying this correction, we obtain a second approximate altitude. To this result apply the corrections from Tables III. and IV., and, if necessary, from Table V.

Example.

M. Humboldt made the following observations on the mountains of Guanaxuato, in Mexico, in latitude 21°, viz.: —

	On Mountain.	Level of Sea.
Barometer, . . .	23.660 inches.	30.046 inches.
Temperature, . . .	70°.3	77°.5

Computation.

Table I. gives for 30.046 inches . .	27649.7
Table I. gives for 23.660 " . . .	21406.9
Difference, ...	6242.8
Table II. gives for 7°.2	—16.9
Approximate altitude, A, . . .	6225.9
$\frac{A}{900} \times 83°.8 =$	+579.7
Second approximate altitude, . . .	6805.6
Correction from Table III.	+13.3
Correction from Table IV.	+19.3
Height above the sea, ...	6838.2 feet.

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3. Project of a Geographical Department of the Library of Congress. By Lieut. E. B. Hunt, Corps of Engineers, U. S. A.

The present occasion seems peculiarly appropriate for bringing forward and initiating a plan, or project, which has been for some months maturing in my own mind, and which has been well received by those to whom I have mentioned it. I trust I do not exaggerate its merits, or overrate its importance, in anticipating that this Association will give it the whole weight of its influence and indorsement. Having occasion, as an assistant on the Coast Survey,

to make various researches into special points of our coast geography, I found it a matter of great difficulty to collate the various authorities bearing thereon, and still more difficult to make sure that I had not omitted some such authorities, possibly of the first importance. It then occurred to me to attempt the collection and methodizing of information relative to maps and charts, and of the localities where they may be found. Something was done in this way; but it was soon clear that a formidable difficulty would still remain in the dispersion of the materials thus indexed. To remedy this in part, some tracings and printed copies were added to the Coast Survey collection, though the limited means thus applicable prevented anything bordering on completeness, even in regard to our own sea-coast. At this stage of affairs it occurred to me that a complete and special geographical library, not only of materials on the United States sea-coast, but of those relating to the whole country, to America at large, and to the whole world, would be a highly valuable aid to all such researches, whether undertaken for the Coast Survey, or for any other purpose, either of history, of science, of commerce, of home policy or foreign relations. This idea at once connected itself with the Congress Library, as the place of all others where it could best be executed, and where it would prove of most value and convenience. Congress is reinstating the library after its burning, and now is a time when this plan can best be undertaken. These geographical aids are greatly needed in Congress for the clear understanding and discussion of many important questions, both domestic and foreign, and in no other place could such a collection better serve all interests. The definite plan which seems to me most worthy to be adopted is essentially the following: —

1st. Let a geographical department of the Congress Library be established as a distinct and independently organized department, with its own executive officer, the general direction being in the Joint Library Committee.

2d. Let special appropriations be made for this department, or let the Library Committee set apart a portion of the general library appropriations for this purpose. During the period of collecting the great mass of existing materials, these appropriations would require to be proportionably large.

3d. Let the appropriated funds be applied to the collection, ar-

rangement, and indexing of all important geographical materials relating to the whole world; also, in part, to the necessary expenses of administration.

4th. Among the materials thus to be collected the following classes may be mentioned: — 1. A first-class terrestrial globe. 2. All material illustrating the early and recent geography of the United States, both its sea-coast and interior, including traced copies of all valuable maps and charts in manuscript, and not published. The materials for illustrating the past and present geography of each State, county, township, and city should be gathered by purchase, correspondence, and tracing. 3. All maps and charts on the remainder of America. 4. The admiralty or sea-coast charts of all the European and other foreign states, and the detailed topographical surveys of their interiors, where such have been made. 5. The most approved maps published from private resources, whether as atlases, nautical charts, or naval maps, including publications on physical geography, guide-books, railroad-maps, and city hand-books. 6. A complete collection of all the narratives of voyages of discovery and exploration, especially those undertaken by the English and French governments. 7. Geographical, geodetic, and nautical manuals and treatises, with all the requisite bibliographical aids to the amplest geographical investigation.

5th. Having an ample organization and appropriation for gathering such a mass of material, it would be of the first importance to arrange complete and systematic indexes or catalogues, which would at once make known all the material in each locality, and to have these materials so arranged as best to facilitate special research.

6th. A drawing-room, in which materials for the collection could be copied, either for its files or to answer public and private calls, would be indispensable for the completeness of this scheme. In this room compilation would be conducted in answer to Congressional calls, and in keeping constantly corrected and filled out a set of State maps on large scales, to which map publishers should have free access.

7th. A competent executive officer would be able to maintain correspondence with persons having special geographical knowledge, and to keep a list of persons who could be addressed for additional information on foreign and domestic localities. Also corresponding relations should be maintained with foreign geographical societies, and their publications secured with promptness.

8th. The head of this department could present, through the Library Committee, an annual report on the geographical explorations by our own and foreign governments, or by individuals, so far as their results could be learned; making this, indeed, a synopsis of all the interesting and important geographical facts or publications for the year.

9th. Among the duties which would belong to this department would be that of calling attention to points demanding examination, or localities needing exploration. Also, it would be able to furnish the preliminary information for such explorations, or to indicate the sources whence it could be derived.

Any one who will reflect on the outline now presented must, I am sure, concede that here is a field, hitherto unoccupied among us, wherein much good can be done, by one possessing the proper qualifications, and that these qualifications must be eminently special if successful results are to be attained. No collection in the United States approaches to the organic completeness or efficiency here contemplated. The Harvard collection, so excellent in old maps, is very deficient in those great works of interior and exterior survey which characterize the last fifty years. No collection exists in our land which furnishes full materials for extensive investigations, such as are now more and more demanded by questions of history, science, commerce, and policy. There is no probability that such a collection can soon be formed anywhere but in the Congress Library. As Congress has so much to gain by this plan, and as the several executive departments at Washington would find so great an advantage in such a systematic collection, it should really be made a national undertaking. In the facilities it would furnish the State Department, the Engineer and Topographical Engineer Bureaus, the Coast Survey, the National Observatory, and the several Naval Bureaus, the government would derive a full equivalent for all its cost. The value of such a collection in its relations to legislation, in its illustrations of river and harbor questions, in its prospective use for illustrating history, and generally as a means of exalting and correcting our geographical knowledge, gives it most truly the character proper for a national enterprise. Nor need we doubt the liberal action of Congress, if the subject be well and earnestly brought before it. All intelligent members must at once perceive its advantage and convenience to themselves in discharging their high trusts. There is scarcely

a day of Congressional session without some question of home or foreign localities so coming up as to need full and correct geographical resources. Moreover, Congress is always well inclined towards actual surveys and explorations, and it would be peculiarly inconsistent for it to lack liberality in appropriating the comparatively trifling amount needed to bring together and arrange the published and manuscript results of such surveys. There appears on the whole no valid objection to the plan proposed, and no reason to question that it can be really executed, if those who know how to appreciate it will exert themselves somewhat, as is always requisite for the attainment of even the least questionable improvements. Hoping that this Association will cordially approve the views now presented, and be ready to act in favor of their realization, I will now respectfully submit the following resolution : —

Resolved, That the President of this Association be requested to appoint a committee of five members, to prepare and present, in the name of this Association, a memorial to the Joint Library Committee of Congress, urging on it, and through it on Congress, the advantages of establishing a complete, thoroughly organized, and liberally sustained geographical department of the Congress Library, and presenting therein such a project or plan of organizing this department as shall seem to the committee best adapted to promote its final usefulness and success, in relation both to the government and to the country at large.

IV. BOTANY.

1. An Account of Six New Species of Plants. [First published in the Class-Book of Botany.] By Professor Alphonso Wood, of Cincinnati.

I embrace the present favorable opportunity for presenting to the lovers of botanical science a more definite account of certain plants supposed to constitute new species, discovered by me in various botanical excursions, while engaged in the preparation of the above-named work.

I. *Gerardia Skinneriana:* caule erecto, gracili, cum quatuor angulis leviter alatis, et cum brachiis remotis; foliis remotis, linearibus, utrinque acutis, quam pedunculi aut internodi ter brevioribus; calycis dentibus perbrevibus, glandulo-acutis; corollæ infundibuliformis campanulatæ lobis brevibus et patentibus; capsula rotund-oviformi, calyce vix longiori.

In barrens, Scaffold Prairie, Green County, Indiana. Root annual. Stem 12 to 18 inches in stature, slender as the grasses among which it grows, rough on the slightly winged angles. Leaves and branches in remote pairs, the latter few, and rising higher than the main stem, the former 6 to 10 lines long and nearly 1 line wide, with a prominent midvein, revolute margins, and much shorter than the internodes. Peduncles 1 to 1½ inch long, erect, very slender, few-flowered. Calyx very short. Corolla 5 or 6 lines broad, glabrous, light purple or rose-colored. Capsule between ovoid and globose, slightly exceeding the calyx-teeth, containing about 30 seeds.

I discovered this delicate plant, July, 1846, in a grass field near the borders of a woodland, on the estate of Dr. A. G. Skinner (whose name it bears, in commemoration of his ardent pursuit of science, and of his important aid rendered me in my early and later botanical studies). Its locality extended over but a few square rods of ground, and I have never met with it or heard of it elsewhere. Subsequently, it has become extinct even there. It is to be sought for in the southern parts of Illinois, in Missouri, &c. I am not sure that Mr. Bentham had this plant in view in his account of either *G. setacea*, or *G. aphylla.* Certainly Mr. Nuttall did not. It more nearly resembles *G. setacea*, var. *parviflora*, Benth. It differs from all these, however, in the form and length of the capsule, in form and arrangement of the flowers, very especially in the leaves, and in retaining its colors completely in drying.

II. *Scutellaria rugosa:* caule erecto, a basi decumbente, ramosissimo, pubescenti; foliis ovatis vel ovalibus, rugosis, pubescentibus, petiolatis, utrinque obtusis, subcordatis, crenato-serratis; racemis simplicibus, elongatis, ramos caulemque terminantibus, bracteis late ovatis, petiolatis, subcordatis, vix calyce longioribus.

On the rocky shores of the Shenandoah, at Harper's Ferry, discovered in 1846, September. A rough-looking, diffuse, and bushy plant, about one foot in height. Stem with the angles obtuse and

the sides grooved. Branches long, curving upwards, and finally erect. Leaves quite numerous, conspicuously rugose, hirsutely pubescent, 12 to 13 lines long, 9 to 13 wide, crenate-serrate, with the ends obtuse (the base often truncate or subcordate), scarcely longer than the petioles, abruptly reduced to bracts which are 2 to 4 lines in both length and width. The form of these bracts differs remarkably from that of the leaves, being broadly ovate, entire, abruptly acuminate, subcordate, petiolate. Racemes simple, terminal, finally 8 to 10 inches long, 40 or 50-flowered. Corolla suberect, $\frac{3}{4}$ inch long, white below, blue at the summit. Carpels black, papillose. July – September.

This species is allied to *S. pilosa*, Michx., but remarkably different in habit, aspect, form, &c. The bracts exactly resemble in form the leaf of the common Lilac. I have been very cautious in admitting this plant to the rank of a new species, on account of its locality; for it seems scarcely credible that it should have hitherto escaped detection in a region so often traversed by botanists.

III. *Sabbatia concinna:* caule gracili, subquadrangulari, internodiis folia 2 – 4-plo excedentibus, ramis oppositis suberectis; foliis linearibus vel lanceolato-linearibus, acutis, sessilibus, 1-nerviis, imis ovatis; panicula oblonga; calycis segmentis linearibus, tubo duplo longioribus; corolla 5-partita, segmentis oblongo-obovatis, obtusis, roseis, stella centrali noatis.

Common in dry, grassy prairies, Indiana. Stem about 1 foot in stature, simple, bearing at top an opposite-branched oblong panicle of few or many flowers. Leaves 10 to 15 inches long, 1 to 3 lines wide, generally one third as long as the internodes. Flowers 15 lines broad, of a delicate bluish purple, the central star yellow, with a green border. Some specimens are found with small oval leaves, scarcely one fourth as long as the internodes.

This beautiful plant is intermediate between *S. corymbosa*, Baldwin, and *S. angularis*, Pursh. It must not be referred to either of these species, unless we conclude to unite the three in one. From the former it differs in inflorescence, in the form of the petals, sepals, and strikingly in the markings and colors of the corolla; from the latter, by its slender habit, internodes, leaves, calyx, and the form and color of the corolla.

IV. *Rumex altissimus:* glaber, erectus; foliis anguste lanceolatis,

integris, petiolatis, utrinque angustatis: racemis gracilibus paniculatiis, nudis, verticillis contiguis; floribus omnino hermaphroditis; sepalis lato-cordatis, acutis, venosis, una vel duabus tuberculatis, interiore semper nuda.

In marshy prairies along creeks, in the valley of the Wabash. Root perennial. Plant tall, 3 to 6 feet high, smooth and showy. Stem striate, slightly branched above. Sheaths short, torn. Leaves thick, 3 to 5 inches long, $\frac{1}{4}$ to 1 inch wide, with a prominent midvein, somewhat acuminate, and tapering to the petioles, which are 1 inch long. Panicle 6 to 10 inches long, leafless. Verticils crowded, forming dense racemes. Pedicels filiform, finally nodding, 2 or 3 lines long. Sepals thin, entire, veiny, pale, the inner broadest and always naked, the second bearing a large tubercle, and the third a small one or none. Fruit exactly resembling a beech-nut in form and color.

I have distinguished this species from *R. Britannicus*, Linnæus, chiefly on account of its narrow, almost linear leaves, and its singly-tubercled calyx. I have never seen any intermediate specimens. Until such be found, I see not how these forms can be united otherwise than by an arbitrary extension of Linnæus's original character.

V. *Potamogeton obrutus:* foliis undatis, nitidis, lanceolato-linearibus, sessilibus, acutiusculis, vena media tantum manifesta, alternatis, approximatis, inferioribus stipulis nullis, superioribus paucis minutis, spicis longo-pedunculatis, acheniis, inflatis, subhemisphæricis, dorso alato, rostra incurvata, utrinque perspicue umbilicatis.

Grows in the slow waters of the Passumpsic River, Lyndon, Vt., wholly immersed, except the flowering spikes. Stems 1 to 2 feet high, slender, terete, simple. Leaves broadly linear, or linear-oblong, 3 to 4 inches long, $\frac{1}{2}$ inch wide, tapering to the slightly clasping base, only the midvein distinct, the two upper opposite and elongated, with the spathe free. Spike $1\frac{1}{2}$ inches long, dense, the peduncle 3 inches long. The achenia are somewhat hemispherical, inflated, winged on the back, the beak incurved, and having a conspicuous pit on each side.

This is a remarkable species, widely differing from any other known to me. The leaves may be said to be intermediate in form between *P. lucens* and *P. Robinsii*, while in the fruit it is more nearly allied to the grass-leaved species. The date of its discovery is

1845. The specific name is borrowed from the circumstance of the plants being always immersed or overwhelmed [*obrutus*] in water.

VI. *Veratrum Woodi* [Robbins] : foliis plerumque radicalibus, lanceolatis et lanceolato-linearibus, glabris, venosis, acutis, plicatis, basi attenuata in petiolum longum alatum et vaginatem ; caule scilicet scapo cylindraceo, procero, erecto, simpliciter paniculata, cum floribus multis qui hìc et illìc steriles, segmentis oblanceolatis, atropurpureis vel ferè atris.

This herb is of a lofty stature, and a singularly striking aspect, growing in shady barrens, Linton, Green Co., Indiana, also in Illinois and Iowa. The root is fasciculate, consisting of numerous thickened fibres. Leaves 10 to 16 inches in length, including the petiole, which is half as long, 3 to 4 inches wide, with prominent longitudinal veins and folds, acute at apex, tapering at base, nearly all of them spreading near the ground. Stem scape-like, with a few remote linear bracts, and of the size of the finger, arising to the height of six feet more or less, paniculate a third of its length. Flowers $\frac{3}{4}$ inch in diameter, nearly sessile, the upper and lower sterile, the six segments greenish-brown outside, blackish-purple, the color of common writing-ink inside, each bearing a red stamen adhering to the base. Ovary oblong, crowned with the three spreading styles of half its length. Seeds compressed, winged, with a broad, loose, membranous testa.

First seen in the above-named locality in July, 1846, and by Dr. Skinner the present season. Dr. Cozzens, of Iowa, has also sent us specimens from that State.

V. ZOÖLOGY.

1. On the Wheat-Fly and its Ravages. By R. Howell, of Nichols, New York.

This fly first made its appearance in Nichols, Tioga Co., N. Y., in the summer of 1850, and has increased yearly since. This is the insect that is generally known by the name of Weevil throughout the country. The Weevil, or *Calandra granaria*, is a bug of very small

size, that only infests grain in the bin, whereas the Wheat Fly, *Cecidomyia Tritici* of some authors, infests the wheat while growing. This fly is undoubtedly of the mosquito tribe, being in shape the same, and one third less in size, of an orange color, with dark-colored wings, or changeable. This fly deposits its eggs undoubtedly soon after the wheat is in bloom, for I found this season, about the 25th of June, when going out to examine the wheat, that the fly was very numerous, hovering around among the grain, and still depositing their eggs; but most of the eggs had, to all appearance, been deposited some ten or twelve days. The egg, or larva, when full grown, is about the eighth or tenth of an inch in length, and of the thickness of a sewing-thread, of a bright orange color, with scarcely any motion. Where there is but one egg to a grain, it seems to do but little damage, often making but a small indentation on one side of the grain. I have found grain in all stages of imperfection, some with large indentations on one side, and large protuberances on the other side, while others were twisted and bent in all manner of shapes, and others were wholly shrivelled and destroyed. Generally, three or four to each grain of wheat destroys the whole. By the time the wheat is harvested, hardly any of the larva are found, and frequently ten or fifteen days before harvest they have nearly all disappeared. A majority of the fields of wheat in this vicinity were damaged but little, only from the edge out in the field twenty or thirty feet. In the northern part of Tioga County, I learn that their ravages prevail to a greater extent than in this vicinity. I learn that is the case in Tomkins and Cayuga County. It is said large fields are only cut in patches. Last season one field in this vicinity was wholly destroyed by this insect. When very numerous in grain, the heads of wheat while ripening will appear of rusty, dark-green color.

The common yellow-bird* feeds on the larva of this insect. One will notice on the edges of the fields of wheat the heads wholly torn to pieces by this bird. I learn that this insect does not trouble the Mediterranean wheat. The Seland and other varieties do not escape. I received a species of wheat from England last year, the Chidham wheat, that is nearly destroyed by this insect, it being a larger and later variety. This insect infests wheat on low, flat, rich land, more than on hilly or gravelly land. It would be well for farmers to sow

* American Goldfinch.

on dry, gravelly, high land, as I have frequently noticed that wheat that keeps green the longest is infected by the insect the most.

Would it not be best, as the members of our Association are from all parts of the Union, to learn when this insect appears and disappears, and to what extent its ravages prevail?

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2. Notes on the Specimens of the Bottom of the Ocean brought up in recent Explorations of the Gulf Stream, in Connection with the Coast Survey. By L. F. Pourtales, Assistant. Presented by Professor Bache, Superintendent.

At the request of the Superintendent of the Coast Survey, Mr. Pourtales examined the specimens of the bottom of the sea obtained by soundings made by Lieutenant Craven, U. S. N., Assistant U. S. Coast Survey, upon thirteen different localities, on the edge of and in the Gulf Stream, off the eastern coast of the peninsula of Florida.

I. Position 2. Latitude 26° 12′; longitude 79° 54′. Depth 500 fathoms. This specimen consists almost entirely of Foraminiferæ, with a very small proportion of quartzose sand, estimated at about ten per cent in bulk. *Globigerina rubra* forms the mass, with a pretty large proportion of *Rotalina cultrata*, *Orbulina universa*, and *Textularia turbo*. It also contains minute Gasteropods (*Natica Nassa*), and fragments of a shell of a crab. The whole is a chalky white color, only a few of the *Globigerinæ* being pink.

II. Position 4. Latitude 27° 37′; longitude 79° 19′. Depth 600 fathoms. Has the appearance of fine white mud mixed with yellow sand. It is composed entirely of Foraminiferæ and their fragments in the form of a fine powder. No silex; acids dissolve the whole, leaving a flocculent greenish matter, probably animal matter. The species are the same as in the preceding, with the addition of *Marginulina Bachei* (rare), *Rotalina Ehrenbergii*, a *Biloculina*, and a crimson *Rotalina* (both rare); also small shells and the habitation of a worm composed of agglutinated Foraminiferæ.

III. Position 5. Latitude 28° 4′; longitude 80° 13′. Depth 15

fathoms. Coarse yellow sand and broken shells. Equal proportions of both.

IV. Position 6. Latitude 28° 21′; longitude 80° 10′. Depth 20 fathoms. Dark gray sand and broken shells; very little quartz. The black parts of the sand make a dark gray mark when crushed, and are dissolved with effervescence in acids, — probably a limestone.

V. Position 8. Latitude 28° 21′; longitude 79° 52′. Depth 100 fathoms. Fine dark gray mud, composed in part of very fine quartzose sand. Of organized beings it contains a bivalve shell, a few very minute *Rotalinæ*, spiculæ of sponges and so-called infusoria of the genera *Coscinodiscus* and *Triceratium.*

VI. Position 11. Latitude 28° 24′; longitude 79° 33′. Depth 1,050 fathoms. Composed of Foraminiferæ, siliceous sand in almost imperceptible quantity. A small portion taken from the lower part of the specimen, after shaking it in water, only showed one or two per cent of siliceous sand, after dissolution in acid. *Globigerina rubra* (white, yellow, and pink, the two first colors predominant) forms the greater bulk. Also *Orbulina universa*, *Rotalina cultrata*, *Bayleyi*, and *Ehrenbergii.* Of other animal remains there were found pieces of coral (Caryophyllia, some white and worn, and some brown and in better condition), a piece of a large Gasteropod, old and worn; pieces of Anatifa, and very small Pteropods (Spiratella).

VII. Position 14. Latitude 29° 22′; longitude 79° 59′. Depth 150 fathoms. Same kind of bottom as No. V. Contains a few *Biloculinæ.*

VIII. Position 15. Latitude 29° 43′; longitude 80° 37′. Depth 19 fathoms. Coarse quartzose sand and broken shells, like No. III.

IX. Position 17. Latitude 29° 50′; longitude 80° 6′. Depth 300 fathoms. Mud like No. V. and VII., contains *Globigerinæ.*

X. Position 20. Latitude 29° 48′; longitude 79° 31′. Depth 560 fathoms. *Globigerina rubra* and *Rotalina cultrata* in about equal proportions. No quartzose sand or other material.

XI. Position 21. Latitude 29° 48′; longitude 79° 17′. Depth 450 fathoms. *Globigerina*, *Orbulina*, and *Rotalina* (*R. cultrata*). No quartzose sand. It contains also a considerable number of very delicate shells of Pteropod mollusks, belonging to the genera *Hyalæa*, *Spirialis*, and *Spiratella.*

XII. Position 27. Latitude 31° 2′; longitude 79° 35′. Depth 15

fathoms. A mixture in about equal proportions of *Globigerinæ* and black sand, probably green sand, as it makes a green mark when crushed on paper.

XIII. Position 32. Latitude 30° 50′; longitude 78° 49′. Depth 550 fathoms. *Globigerina*, *Orbulina*, *Rotalina* (*cultrata*, *Ehrenbergii*). Small shells (*Spirialis Gouldii*, *Skenea?*). Small spines of Echini. No quartzose sand.

Four specimens were also obtained by Lieutenant-Commanding J. N. Maffitt, U. S. N. Among them Specimen No. 2, from a depth of six hundred fathoms in latitude 31° 32′, and longitude 78° 20′, presents some interest. It consists in Foraminiferæ and small shells, and in fragments of shells and corals (Cyathophyllia). The Foraminiferæ are chiefly large specimens of a kind of *Rotalina* of a rough and heavy appearance. Among the shells a small *Concholepas* appears quite abundant. The fragments of shells and corals appear rounded and worn, indicating perhaps an agitation of the water by a current near the bottom.

Specimen No. 4, from a depth of four hundred and sixty fathoms, in latitude 30° 41′ and longitude 77° 3½′, consists of the usual deep-sea Foraminiferæ, with small shells and dead corals, and fragments of dark stone.

A few interesting remarks suggest themselves from the above examination, but before passing over to them, the following question has to be answered. Are these small animals actually living in the localities from which they were obtained, or have they been gradually washed down from the reefs near which the current has passed? I feel inclined to answer that they were living when found, from the fact that the greater number of the individuals are perfect, notwithstanding the great delicacy of their shell. The delicate pink color of the *Globigerinæ* would scarcely be preserved in specimens transported from a distance. The best argument in favor of this opinion is perhaps the fact, that the same species are found in a perfect state as far north as the coast of New Jersey and New York. It is very singular, however, that the same species should also be found living on the shores of Cuba and of some of the other West India Islands, under exceedingly different circumstances of light and temperature.

If we admit them living at the great depths where we have found them in such abundance, we are enabled to extend the limits of ani-

mal life to a much greater depth than is generally admitted. Professor Edward Forbes, in his report on the distribution of Mollusca and Radiata in the Ægean Sea, (Reports of the British Association, 1843,) supposes that in depths beyond three hundred fathoms animal life does not exist. In a former report on this subject, (American Association, Charleston,) I remarked that the *Globigerina rubra* seemed to increase in abundance with depth. I had then seen specimens from depths not exceeding two hundred and sixty-seven fathoms, and its greatest abundance did not exceed about fifty per cent of the mass. We have now found a maximum of its occurrence at a depth of one thousand and fifty fathoms, where this and allied forms constitute the entire bottom. It is but reasonable to suppose that still deeper explorations would show a gradual decrease for a considerable depth before it should cease to appear, as was shown from other species living in more shallow water, in the report alluded to.

In concluding, I would remark, how important a knowledge of the habits and distribution of the Foraminiferæ is for geologists, since, of all classes of the animal kingdom, none has contributed a larger share to the formation of rocks, at least in the cretaceous and tertiary formations.

VI. PHYSIOLOGY.

1. On the Formation and Mode of Development of the Renal Organs in Vertebrata. By Dr. W. I. Burnett, of Boston.*

There are two facts strikingly indicative of the importance of the urinary organs in all the higher forms of animal life. These are, first, their widely distributed presence ; and second, their early appearance in developing embryonic forms ; I might, perhaps, add as a third fact, that their functional activity is usually in pretty exact ratio with the grade of organization.

Throughout the higher classes of the Invertebrata,† and in all the

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* This paper was illustrated by numerous large diagrams or figures, representing the formulæ and line of development of the organs in question.

† Urinary organs are not found lower than the Cephalophora, in which, as also

Vertebrata, these organs are present, and their functional relations quite prominent; it is only in the lower classes of the animal kingdom, where there is an absence or incompleteness of a true circulatory system distinct by itself, that these structures are wanting. The kidneys are organs inseparably connected with an important blood-function, and in the Vertebrata, where the conditions of organization rest upon an active and widely-distributed circulation, they possess determinate anatomical and physiological characteristics, and may therefore form the subject of a distinct and complete investigation.

On this account I limit myself in the present paper to an examination of those organs as observed in the four grand classes of Vertebrata, — Fishes, Reptiles, Birds, and Mammals; and by the terms *Urinary Organs*, I mean those both of a transient and permanent nature, — the Wolffian bodies of most embryos, as well as the true kidneys of all adults.

I have lately enjoyed excellent opportunities for the study of the development and intimate structure of these glandular organs; and their apparent peculiarities, as observed in the four different classes, require the detail I have given, in order to convey a clear idea of what may be called the *formula* of their organization. I say formula, for the intimate organic structure of a urinary organ, wherever observed among the Vertebrata, is invariably the same; the variations being only apparent and extrinsic, and due to the many modes of combination of a single individual structure.

As before mentioned, the renal organs appear under two forms, namely, the Embryonic, or the so-called Wolffian Bodies, and the Permanent, or true Kidneys.

I. *Wolffian Bodies.*

To the physiologist it is a beautiful and suggestive fact, not unfrequently observed in embryological studies, that nature sometimes puts up a temporary, provisional structure for the performance of an important function, until the conditions of the general organization shall be so far advanced that there can be formed a permanent organ of a

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in the Cephalopoda, Crustacea, Arachnoidæ, and Insecta, they assume an important function, and are often of a complex structure. See Comparative Anatomy, by Siebold and Stannius, transl. &c. by Burnett, Vol. I. §§ 223, 255, 288, 314, 345.

certain type, belonging to the animal as such, and which persists through its entire life. Such a fact is presented by the Wolffian bodies.

In the higher forms of organization, the blood, directly upon its active circulation, seems to require some means for the removal from it of certain effete particles, and to effect this, a delicate, transient structure is erected, to remain only until a permanent one in the shape of a true kidney can be formed.

These temporary kidneys are found in the embryos of all the Vertebrata, excepting the Fishes and the Amphibian Reptiles; that is, in the true Reptiles, the Birds, and the Mammals. The length of time they persist is, in general, in an inverse ratio to the grade of the animal; in fact, this law of gradation seems so marked, that there would at first seem some ground for the opinion, that in the Amphibia and the Fishes, which have only permanent renal organs, these last may be only persistent Wolffian bodies; but this point will receive our attention at a future time.

Wherever occurring, these Wolffian bodies present the same unvarying form and type of structure; their mode of development I have found to be equally invariable, whether occurring in Reptiles, Birds, or Mammals. They always make their appearance during the very earliest phases of development of the embryo, and, with the exception of the heart, are the first organs formed in the abdominal cavity.

As I have studied these phases of development in Birds more carefully than in the other classes, I will describe them as observed in the chick; and this description will include what belongs to these organs in Reptiles and Mammalia, in all their essential details.

In the chick, there appears, at about the fiftieth hour of incubation, a line on each side of, and lying close to, the vertebral column; this line extends from near the region of the heart to the caudal vertebræ, and is composed of a collection of nucleated cells which soon become arranged so as to form a tube; this tube is the basis of the future Wolffian body. At this stage of development, there is observed, then, a simple tube on each side of the vertebral column; but a few hours after, the surface of the tube becomes nodulated at regular intervals on its inner surface. These nodulations are the beginnings of a series of eversions from the main tube, which

soon, therefore, has a digitated appearance, — each of the finger-like prolongations being a future uriniferous tube. These prolongations having been formed from without inwards, the original tube, which now has become a common duct, lies upon the external side, and the former overlap the vertebral column.

This formation of tubes by diverticula from a main one, constitutes the first phase in the development of this organ as a compound structure. The second phase is the formation of a direct connection between the blood-vessels and these newly-formed tubes, so that an eliminating function may be performed. This occurs at about the sixtieth or sixty-fifth hour of incubation. At this time, the free extremities of some, but not all, of the newly-formed tubes become enlarged and dilated into an infundibuliform body, which, together with its attached tube, resembles a flask with a very long neck. *In* this dilated extremity of the tube, a knot of bloodvessels is formed anew from epithelial cells; this occurs in a manner so beautiful, that I digress here for its more careful description.

This infundibuliform or bulged end of the tube is nothing but the tube at this point taking on a little larger growth; it is therefore lined, like the tube itself, with a layer of epithelial cells. These cells, by a more or less linear arrangement, form a minute convoluted tube, enclosing in its calibre smaller cells; this tube is the future bloodvessel which connects afterwards with the bloodvessels of the general circulation, and then the enclosed epithelial cells become blood-corpuscles. This convoluted bloodvessel is the so-called Malpighian tuft, or the *glomerulus*, and is the functional or active structure of the organ.

A brief but comprehensive description of the structure of the Wolffian body would then be: a straight, main duct, into which empty many digitiform tubes, the capsular dilatations of the free ends of which contain each a knot of bloodvessels. In the same way, I might describe the method of its function, as the straining off from the blood, through this knot of vessels, those effete particles which, as a whole, form the urinary excretion of the embryo. A structure more simple cannot easily be conceived of, yet its function is most effectual; for although thus quickly formed, we shall soon learn that both the structure and function of the complex permanent kidney rests upon precisely the same primitive types.

These temporary kidneys, thus formed, which have, as before re-

marked, exactly the same structure wherever found, have a duration varying in the different classes, which stands in an inverse ratio to the grade of the animal.

In the true Reptiles, and in Birds, they persist as active functional organs during a considerable portion of the embryonic life; in fact, their disappearance coincides with the appearance of the kidney as an active structure. Their structural remains, however, are often found long after the animal has passed from its dependent, vitelline life; thus, in the chick, the kidneys assume the urinary excretion at about the tenth day, yet the vestiges of those bodies may often be observed after hatching; and in the alligator I have seen, even four or five months after the animal had escaped the egg, the remains of these organs so well preserved, that the Malpighian bodies were distinct in them. But in the Mammalia, where their existence as active parts is very brief, in fact so limited it is difficult to observe them in a state of functional activity, they are correspondingly soon absorbed; and although these remains are observed more or less distinctly after birth in some species, yet generally they have mostly passed away by the latter half of the intra-uterine life.*

As I have shown elsewhere,† the receptacle of this urinary excretion of the Wolffian bodies is the allantois, which, as I have described

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* Müller (Physiologie, transl. by Jourdan, &c., deux ed., Paris, 1851, II. p. 760, fig. 290, C. 5) has given a figure representing the remains of the Wolffian body in a human fœtus of $3\frac{1}{2}$ inches. As is well known, there may be observed during the last months of the human fœtus, or even after birth, peculiar canaliculi situated in the fold of the peritoneum and of the Fallopian tube. They form the so-called organ of Rosenmüller (De Ovariis Embryonum, Leipzig, 1801), and are probably the remains of the Wolffian body. In the Ruminantia, Solipeda, and Suina, there are likewise canaliculi (the canals of Gaertner), with thin walls, which extend from the large ligament of the uterus along the neck of this organ, pass between the mucous membrane and the muscular tunic of the vagina, and open, at last, near the urethral orifice. These, Jacobson (Die Okenschen Körper oder die primordial Nieren, Kopenhagen, 1830, p. 17) thinks, are the remains of these same bodies; a view regarded as probable likewise by Rathké, Valentin, and Gurlt, but which certainly requires new proofs. See, in this connection, Rathké in Meckel's Arch., 1832, p. 386; Valentin, Handbuch der Entwickelungsgeschichte, &c., Jourdan's edit., p. 348.

† See a paper *On the Formation and Function of the Allantois*, in the Proceed. Amer. Acad. of Arts and Sciences, Boston, for October, 1852.

in the paper referred to, is at first properly nothing but their receptacular appendix, — the excretory ducts terminating in it below.

In conclusion, I may remark that the Wolffian bodies are transient in all their relations; they subserve nothing for the formation of the permanent organs that succeed them, being in every way distinct structures; and the testicles or ovaries which are first observed on their bodies have no other relation with the part on which they rest except that of mere contact.

But before leaving this section of the subject, I think it necessary to explain one point which otherwise might seem assumed on too little authority. I refer to the statement I have already made, that the Wolffian bodies are absent in the Amphibian Reptiles. This, as is well known to physiologists, is directly opposed to the views of Müller, and therefore demands here a special reference.

Among Müller's earliest anatomical publications upon the formation of glandular structures, and particularly those of the genital apparatus, he pointed out and gave a careful description* of what he regarded as a new structure in the embryos of Amphibia. Up to this time, the so-called Wolffian bodies had not been observed by Meckel and Rathké, who had specially studied them, in either the Fishes or the Amphibian Reptiles; and these newly-discovered structures Müller regarded as a peculiar form of the Wolffian bodies. He described them as two organs situated one on each side of the vertebral column, directly under the branchiæ of the larvæ of Batrachians, and from which proceeds a duct that runs along the side of the vertebral column, and opens, finally, into the lower portion of the intestinal canal.

But the presence of these organs, thus carefully described, and especially by so excellent an authority, has not been acceded to by all, although by most, subsequent microscopical anatomists;† at

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* Bildungsgeschichte der Genitalien, Dusseldorff, 1830. See, also, his De Glandularium Secernentium Structura Penitiori earumque prima Formatione, 1830, p. 86, Tab. XII., and his Physiologie, transl. by Jourdan, &c., deux ed., Paris, 1851, II. p. 758.

† Among those who have followed Müller may be mentioned H. Meckel (Zur Morphologie die Harn- und Geschlechtswerkzeuge der Wirbelthiere, Halle, 1848), and Reichert (Das Entwickelungsleben im Wirbelthier-Reich, 1840, p. 26).

least that they are distinct organs, as independent as the permanent kidneys, or as the ordinary Wolffian bodies of Birds and Mammals.

This is a point to which I gave special attention in making these investigations, and after the most careful and repeated examinations of the larvæ of Batrachia, in all their stages, I have wholly failed to find a structure, such as Müller has described, distinct from the developing kidney, and which would correspond to the ordinary Wolffian body. On the other hand, all that I have observed in this respect is, that the common duct (future ureter) of the forming kidney extends quite high up towards the cardiac region, some distance beyond the upper limit of its branching out into uriniferous canals. The upper end of this free portion of the duct is convoluted, and seems to have some direct connection with the bloodvessels, though I have never here observed anything like Malpighian bodies. As the development of the kidney progresses, this free extremity of the tube gradually disappears, and towards the end of the larval state is observed only in remains. In brief, then, I have observed no distinct, temporary urinary organs in the undeveloped forms of Amphibia.

These observations, made for the most part during the summer of 1852, though repeated from time to time until quite recently, I was pleased to see confirmed by so excellent an observer as Wittich, in a paper published in the autumn of the same year.* Wittich's investigations have been very extended, and, as far at least as they relate to the renal organs, furnish results much like my own. He found nothing in the larvæ of the naked Amphibia which would correspond to Müller's Wolffian bodies, excepting the free prolongation upwards of the main duct, or the ureter of the developing kidney.

The doctrine here insisted upon, that the true Wolffian bodies, or fœtal kidneys, are present only in the embryos of the true Reptiles, the Birds, and the Mammals, is so apposite with the results I have obtained from an investigation of the allantois, that it may be urged with an added force. In the paper already alluded to, I have sought to show that the allantois is, primitively, the vesicular expansion of

* Beiträge zur morphologischen und histologischen Entwickelung der Harn- und Geschlechtswerkzeuge der nackten Amphibien, in Siebold and Kölliker's Zeitsch. f. wissensch. Zoöl., IV. 1852, p. 125; also, Harn- und Geschlechtsorgane von Discoglossus pictus und einiger anderer aussereuropäischer Batrachier, Ibid., p. 268.

the combined extremities of the ducts of the Wolffian bodies, and finally becomes the receptacular appendage of these organs, — its contained liquid being properly urine.* But afterwards it subserves also another function, — that of the aeration of the blood.

This view being correct, we could expect to find Wolffian bodies only where there is observed an allantois, and *vice versâ*. Thus, in the Fishes and Amphibian Reptiles, which truly have no allantois, there would be no temporary kidneys, or the Wolffian bodies; while in the other Vertebrata, which are allantoidian, these last would be found. I scarcely need refer further to this parallelism as far as regards our doctrine in question.†

II. *Permanent or True Kidneys.*

These organs, being apparently indispensable to the adult economy of all the Vertebrata, have a physiological importance of the highest character. Although the primitive essential type of their structure is precisely like that of their temporary analogues just described, when present, yet as they are permanent organs, and sustain definite relations and connections with various parts of the general system, the study of their structure as organs is the more complex and interesting. It would be foreign to my subject to enter upon the details of the com-

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* Jacobson, as is well known, detected urea in the liquid contents of the allantois of very young embryos. See *loc. cit.*

† I much regret that, in preparing this account of the Wolffian bodies, I should not have had direct access to the work of Follin (Recherches sur les Corps de Wolff, Paris, 1850), which I have failed to get, on the reputed ground of its being out of print. Considerable reference, however, is made to this work, by the copying of some of its figures, in Littré's edition of Jourdan's translation of Müller's Physiologie, Paris, 1851, II. p. 761. But by the figures in question I can judge nothing of the views of M. Follin upon this structure.

Besides the well-known works of Rathké, Meckel, Baer, Jacobson, Valentin, and Bischoff, already referred to, see especially, for representations of these bodies, Müller, Bildungsgeschichte der Genitalien; his De Glandularum Secernent., &c. and his Physiologie, transl. by Jourdan, Paris, 1851, II. p. 757. For figures of the so-called Wolffian bodies of Amphibia, see Wittich, *loc. cit.*, Taf. IX. figs. 1, 2, 3, 4, 5, 6, &c.

The general structure of these bodies has long been known, and I am not aware that I have added any new facts in this respect. My observations relate chiefly to the very first stages of formation of all the parts, and concerning this have furnished results I have seen recorded by no investigator.

parative anatomy of these organs, describing those variations of external form belonging to each type.* I shall limit myself to those points that bear directly upon the subject of development and intimate structure.

As a leading fact, I may mention that the idea or *formula* of the kidney, wherever observed in the vertebrate, is always the same; and thus finding the intimate structure essentially alike everywhere, we should naturally infer that there is throughout a single and invariable mode of development. This, indeed, I have found to be the case, as I hope to show in the following details.

In the Fishes and Amphibian Reptiles, where there are no temporary urinary organs, the earlier phases of development of the kidneys seem to be a little less complicated than in the higher classes. In fact, the type of structural development here is quite allied to that of the Wolffian bodies, and by some anatomists these organs in Fishes have been regarded as merely permanent forms of the above-mentioned bodies. This last, however, I do not think correct, for in this class the subsequent phases of development, by which the organ is increased in size, are exactly like those of the general formation of these organs in Birds and Mammals.

The formula of development is the branching out indefinitely of a primitive tube, which is the future ureter; — the terminal portions of these branches forming an intimate connection with the general vascular system, either by Malpighian bodies, or by a delicate net-work of vessels.

The mode of formation is therefore arborescent, and the analogy of the development and growth of this gland with that of vegetable forms, is too striking not to be noticed. We shall see that the growth almost exactly resembles that of a tree, even in its details.

The line of development being everywhere the same, I will, for the sake of uniformity, describe it as occurring in the chick, where, moreover, I have carefully registered the successive phases.†

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* For full details on this subject, with a copious reference to its literature, see the Comparative Anatomy of Siebold and Stannius, transl. &c. by Burnett, II. §§ 49, 106, 153, 260.

† Here I may perhaps well allude to the views of other observers who have specially studied the development of the kidney. The authors best known in this respect are Rathké, Müller, Valentin, Bischoff, as well also as Wittich of later times.

In the chick no traces of the kidney are perceptible, according to my own observations, until the end of the fourth or the beginning of the fifth day. The ureter, then, is the part first seen, and consists of a simple tube, the upper part of which sends off branches. Each of these branches then divides and subdivides, and around these subdivisions are collected a mass of blastematous cells. These cells serve as the material for the growth and further divisions of the tubes, — in stem (ureter), which has many branches, each of which is the footstalk of a leaf-like body; this last is one of the future lobules of the kidney. The whole plan of structure is here laid out, — a branching ureter, with lobules. The increase of the size of these lobules, and the formation of the ultimate uriniferous tubes, take place by one and the same process; namely, by the branching of the original tubes in a plumose form from a main tube, until the whole structure is completed. There is observed, therefore, at an early period, a main of a

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The works of these men have already been cited. According to Rathké, the first traces of the kidney consist of a number of small claviform protuberances appearing in an amorphous blastema, situated in the place of the future kidney. These, he says, are pediculated, and these pedicles ultimately become attached to the ureter, which at this time is not yet formed. In these claviform bodies the uriniferous tubes subsequently appear, but in a manner not precisely determined.

Valentin's investigations agree in general with those of Rathké; but, from some observations made upon very small embryos of the hog, he concluded that the ureter, pelvis, and uriniferous tubes were primitively developed separately, being subsequently united. Valentin's view of the formation of the uriniferous tubes coincides with that of Henle, namely, that it occurs by the condensation of a linear arrangement of the vesicular contents of the blastema.

Both Müller and Bischoff confirm, in general, the views of Rathké; but Bischoff differs not a little upon some points which deserve mention. He says: "I have never been able to convince myself that the ureter, pelvis, and canaliculi are developed separately, and I think that their rudiments are continuous throughout. They are not hollow at first, and their internal cavity is the result only of further development."

But, of all, Wittich's results most closely approach my own. Speaking of these organs in the Amphibia, he says: "The further growth of the kidney occurs, it would appear, partly by a new diverticulation of the excretory duct (ureter), and partly by a widening, lengthening, and branching of the primitive diverticula."

As will be seen in the text, the view I advance is the arborescent mode of development, more or less marked, according to the class, of these organs, — the formula being a more or less extended diverticulation of tubes from a main tube or duct.

feather. The tubes do not end, however, on the edge of the plume (so to speak), but here loop and return, and when near the shaft or main tube they dilate into Malpighian bodies; this is, as far as I have observed, their invariable mode of termination, there being no anastomoses of the tubes, as some have supposed.

The mode of formation of the Malpighian body of the true kidney is precisely like that of the same structure belonging to the Wolffian body, already described; I need not, therefore, here repeat the description. I may remark, however, that the two bloodvessels which compose the glomerulus usually penetrate the capsular dilation of the tube at some point more or less near the opposite of its insertion upon the tube, and rarely more laterally, or at least on the inner half of the capsule. In the chick, the Malpighian bodies begin to appear about the tenth day; they are then very few in number, but they become more numerous exactly in proportion to the growth of the organ, — not ceasing to be formed until the kidney has reached its full size.

Nothing could be simpler, therefore, than the mode of formation of the renal structure; it is clear and unmistakable, — one tube gives rise to another by an eversion of its walls, and this last produces another in the same way, and so on.

Such is the mode of development occurring in Birds, and my observations upon it, as found in all the other classes, show that the formula is there invariably the same. There are, however, peculiarities of the combination of this formula in each class, which demand a special consideration.

In Fishes, these organs appear at an early period as two straight tubes, extending one on each side of the vertebral column, from the region of the heart tothe anus. These tubes soon give off from their inferior and inner surface *diverticula*, exactly as with the Wolffian bodies just described, and for a time the organs have much the same appearance as embryonic kidneys. But afterwards, as the animal increases in size, a new development takes place. This consists in the branching, dichotomously, of the diverticular tubes, — a process which goes on indefinitely, giving the whole organ much the same lobulated, arborescent structure as that of the chick, — although the plumose arrangement of the terminal tubes is not here present. In both the Cartilaginous and the Osseous Fishes, the functional or se-

creting structure is, as far as I have observed, always the same; that is, the tubes invariably terminate each in a well-formed Malpighian body.

In the Amphibian Reptiles, the kidneys quite closely resemble those of fishes, yet simulating, even more closely than these, the general character of the Wolffian bodies. They present little tendency to an arborescent structure, the tubes of the main duct rarely dividing, but rather forming convolutions, and ending invariably, according to my own observation, in Malpighian bodies.* These Malpighian bodies, together with their adjacent portion of the uriniferous tube, are lined with long lash-like cilia, the ever-constant, rapid action of which, waving towards the outlet, presents a beautiful aspect. The use of these cilia is, evidently, to direct the course of the current out of the Malpighian body, thereby keeping this last in a free, unobstructed state. In the true Reptiles, the development of the kidney is arborescent, as in Birds, and in the Serpents the lobules thus formed remain more or less separate through life. This is also the case, but in a less degree, in the Chelonians.

But the Ophidians present this peculiarity, that all the uriniferous tubes do not end with a Malpighian body; in fact, I have been able to find out but few of these bodies, which are usually situated on the posterior portion of the organ. Many of the uriniferous tubes terminate in a simple cæcal extremity, and here the ultimate connection with the vascular system, by means of a Malpighian body, is replaced by a large system of renal vessels, the indefinite ramifications of which permeate the whole organ, and by very delicate anastomoses bring every such cæcal secreting tube within the influence of a bloodvessel.†

As for these organs in the Birds, I have described their peculiarities in speaking of the chick, and have only to add, that throughout the entire class there seems but little or no variation.

In the Chelonians, the kidneys quite closely resemble, in every point of view, those of Birds. The uriniferous tubes have the same

* For representations of the intimate structure of the kidneys of Amphibia, see, especially, Wittich, *loc. cit.*, Taf. IX.

† There is, I have lately observed, a similar relation between the bloodvessels and the terminal cæca of the poison-gland of the Rattlesnake (*Crotalus*), — a kind of *rete mirabile*.

form of distribution, and terminate always each in a Malpighian body.

In Mammalia, the course of development followed is like that observed in Birds; but the subsequent changes by which the organ becomes more compact often entirely conceal the original forms. By observations upon very young embryos in the different families, it is evident that the development is arborescent, forming lobules,— and these lobules are at first exactly like those observed in Serpents and Birds. In some species, as is well known, these lobules remain distinct through life; this is well seen in the otter, bear, and whale, and in many of the Ruminantia these remains are distinctly visible. It is only in the higher forms that they have so coalesced as to be concealed.

This intimate combination of parts produces the greatest amount of secreting surface in the smallest space. Take, for instance, the kidney of man; here the lobules are arranged in a half-circle around a common cavity, or the pelvis. But they are so united as to become conical in shape (with the bases of the cones at the surface of the organ), thereby producing the so-called pyramids. These pyramids are composed in part of tubes that spread out in a fan-like manner from a common point (calyx), and the gradually increased size of the medullary portion is produced by the branching of the tubes in a dichotomous manner. The straight, regular way in which these tubes run seems to be due, in a measure at least, to the mechanical pressure to which they are subjected by the combination of the lobules; for I have been unable to perceive it in very young embryos, and, moreover, it does not agree with the invariable arborescent conditions of early formation. The so-called medullary portion of the kidney is made up of fasciculi of straight tubes, which divide dichotomously, but have no Malpighian bodies; they continue directly to the cortical portion, which is composed of more or less convoluted tubes,— the result of the dichotomous division. These tubes finally end each in a Malpighian body. These Malpighian bodies sometimes lie upon the surface, directly beneath the capsule; but often also the tubes run up to the surface, loop, return a short distance, and end in a Malpighian body deeper in the renal structure. From all I have observed, it appears that in man the Malpighian body is the *only* termination of the final tubes, there being therefore as many of the former as of the

latter. I have seen nothing like an anastomosis of the final tubes, as some have supposed is sometimes the case. The pelvis of the human kidney, as also that of other Mammals, is formed by the dilation of the main duct, or ureter, involving the primary branches which give off the straight tubes of the medullary substance. In this way, and by the union of the bases of the pyramids, the apices are left free, projecting into this pelvis or main cavity. These changes I have enjoyed an opportunity of tracing in an embryo.

It will be seen, therefore, that the development of the kidney in man and the higher Mammals involves no new phases, — the differences of general structure being extrinsic, and due to combinations which produce compactness and decreased size without a corresponding decrease of functional surface.

In the foregoing account, I have preferred not to burden the general text with a reference to the history and criticism of some of the most important, as well as disputed, points connected with this subject. These can better be discussed by themselves.

The most important of these points is the character, relations, and connections of the Malpighian body. This secreting organ of the kidney had long been recognized by both the older and the more modern observers, but its intimate structure, and its connections with the other parts of the renal substance as the functional secreting organ, were first successfully made out and published by Bowman,* in a memoir which has since become classic on this subject. I may here mention, that it is quite a remarkable historical fact, that, more than fifty years before, Schumlasky † expressed the view that these bodies were the source of the urinary secretion, and had direct connection with the uriniferous tubes. But this view was opposed by subsequent observers. In 1841, Müller ‡ published a portion of his work on the anatomy of the Myxinoid Fishes, where he describes the kidney as consisting of sac-like diverticula from a main tube or ureter, terminating cæcally, and in the end of which is a small tuft or knot of bloodvessels. The value of this discovery was not appreciated until Bowman (ignorant, however, at this time of Müller's investi-

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* Philosophical Transactions, London, 1842, Pt. I. p. 57.

† De Structura Renum. Strasburg, 1788.

‡ Vergleich Anat. d. Myxinoiden, Dritte Fortsetzung, Berlin, 1841, p. 13.

gations on this point) published the next year his memoir. To Bowman, therefore, we seem indebted for the first full exposition of the secreting structure of the kidneys of Vertebrata.

Bowman's doctrine was, that the Malpighian body is the infundibuliform expansion of the uriniferous tube, and that the glomerulus or tuft of bloodvessels lies enclosed freely in this expansion, being composed of a tortuous loop, the two component vessels entering at the same point which is generally opposite, or nearly so, to the point of insertion of the uriniferous tube. The Malpighian body thus composed, Bowman maintained, is the exclusively secreting structure of the kidney. These are the essential features of the results advanced, and I need not enter into the details of a memoir so well known as this.

Results so valuable in physiology were, of course, examined by different investigators on every side. Reichert,* in his Report upon the Progress of Microscopical Anatomy for 1842, enters into a critical discussion of this subject. He denies that the uriniferous tubes end in the capsule, and regards this last as a distinct and separate formation. He also denies the presence of ciliated epithelium in the Malpighian body and the uriniferous tubes. On the other hand, he maintains that the glomerulus, or knot of bloodvessels, is enclosed in the Malpighian body. These views he has defended in some of his subsequent reports.†

In 1845 Gerlach‡ published a still different opinion upon this disputed point of the Malpighian body. He maintained that the uriniferous tubes do not end in flask-shaped cæca, but loop and pass into each other. The Malpighian bodies he declares to be globular diverticula from the tubes, and that they contain, as Bowman advocates, the glomerulus. This author has likewise reasserted his views in a subsequent paper.§

The view of Bidder‖ is even still different. According to this observer, the uriniferous tubes end in flask-shaped cæca, but these

* Bericht über die Fortschritte der mikroskopischen Anatomie in dem Jahre 1842, in Müller's Arch., 1843, p. ccxvii.

† See Bericht, &c. for 1848, in Müller's Arch., 1849, p. 65; also that for 1849, in Ibid., 1850, p. 67.

‡ Beiträge zur Structurlehre der Niere, in Müller's Arch., 1845, p. 378.

§ Zur Anatomie der Niere, in Müller's Arch., 1848, p. 103.

‖ Ueber die Malpighischen Körper der Niere, in Müller's Arch., 1845, p. 508.

last do not enclose the glomeruli. On the contrary, the Malpighian bodies receive each a glomerulus in a kind of depression, and so the cavity of the Malpighian body is as disconnected from the glomerulus as is the cavity of the pleura from the lung.

These, as far as I am aware, are all of the dissimilar views maintained by recent observers. It is indeed remarkable that there should have occurred such a discrepancy. Most other investigators* of note follow more or less completely the views of Bowman. Among these may be mentioned Kölliker,† Patruban,‡ Nicolucci,§ Mandl,|| Victor Carus,¶ and Wittich.**

As to my own views, they will appear sufficiently plain in the foregoing pages. I subscribe to Bowman's results very generally, as to the relations of the Malpighian body to the tubes and to the glomerulus. In tracing the successive phases of the development of these parts, this point enlisted my special attention from its disputed character. The Wolffian bodies are the best structures for study in reference to this point, and, having traced in these the budding out, as it were, of the uriniferous tubes, the dilatation at the extremity into a flask-shaped body, and the formation, in this last, of the Malpighian tuft or glomerulus, I can have no doubt that all these connections and relations really exist.

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* Hyrtl, however, does not follow Bowman, but rather Gerlach; that is, that the uriniferous tube does not end in the Malpighian body. See Beiträge zur Physiologie der Harnsekretion, in the Zeitschrift der Gesellschaft der Aerzte zu Wien, II. p. 381.

† Ueber Flimmerbegungen in den Primordialnieren, in Müller's Arch., 1845, p. 518.

‡ Beiträge zur Anatomie der menschlichen Niere, in the Prager Vierteljahrsschrift, XV. p. 87.

§ Sull' intima Struttura dei Reni in Filiatre-Sebezio. Feb. 1847.

|| Mémoire sur la Structure intime des Organes Urinaires, p. 18.

¶ Ueber die Malpighischen Körper der Niere, in Siebold and Kölliker's Zeitsch. f. wissenschaftl. Zoöl., II. 1850, p. 58.

** Beiträge zur Anatomie der gesunden und kranken Niere, in Virchow and Reinhard's Arch., p. 147.

2. On the Formation and Functions of the Allantois. By Dr. W. I. Burnett, of Boston.

I will not here detain the meeting by a discussion of the historical relations of this interesting subject, but will proceed to the detail of my proper observations.

These were made upon Mammals, Birds, and Reptiles. But as in these three classes there are no essential differences, the phases of formation in Birds, which are most convenient for study, may be described as exponents of the whole.

In the chick the allantois first appears at about the sixtieth hour of incubation. At this early period, the abdominal plates enclose no organs except the heart, with its ascending and descending aortas, and the Wolffian bodies. There is then no trace of an alimentary canal, or any of its appendages.

At this early period, the Wolffian bodies consist of two tubes, one on each side of the vertebral column, running from the region of the heart to the caudal extremity.

From each of these tubes there then project short digitations, which are to be the future uriniferous tubes of this organ; the original tube becoming the duct of them all in each organ.

These ducts pass down to the last caudal vertebra, over which they turn and come together; at their point of junction appears a small vesicle, the expansion of their combined extremities. This vesicle — a minute sphere, and scarcely to be distinguished from the extremities of the ducts themselves — is the *allantois* in its earliest condition.

At first its walls are extremely thin, being of a most delicate membrane; but as its size increases, cells appear upon its inner surface, and at last a basement membrane is perceived, covered with epithelial cells. All these formative changes have taken place beneath the investing membrane of the whole embryo, and directly at the point of the branching of the two umbilical arteries.

As the vesicle expands, it pushes out, first, the branches of these arteries, which rest upon it, and by anastomosis form a network; second, a hood of the membrane investing the whole embryo. In less than a day after this, when the vesicle has attained the diameter

of one sixteenth of an inch, the net-work of vessels united in the hood of the investing membrane has so increased, that it seems to form the vesicle proper, the original membrane being entirely masked. At this period the allantois has very much the aspect of a diverticulum of the investing membrane of the embryo, and to this perhaps is due the opinion of Coste as to its origin.

After this it increases rapidly, the spherical vesicle becoming flask-shaped, and extending out quite beyond the caudal vertebræ, around which it passes to reach the dorsal surface of the embryo. Here it meets the amnion, with the membranes of which it partly blends, and in this way serves to conduct to it the umbilical vessels.

Such is its mode of formation. Its functional relations are equally interesting.

I would remark, in the first place, that the Wolffian bodies are truly depurating organs of the blood; in fact, are the temporary kidneys of the embryo. We have seen that the allantois appears as the bulbous termination of their combined ducts, at a very early period of embryonic life. But it does not arise until the Wolffian bodies have attained a functional power; that is, until uriniferous tubes are formed, having direct relations with the bloodvessels. Indeed, the allantois, as the receptacular termination of the ducts of the Wolffian bodies, is not formed until a urinary secretion is produced.

These facts, joined with the very significant one, that Jacobson found uric acid in the liquid of the allantois at a very early period, seem clearly to indicate that the primary physiological function of the allantois is to serve as a urinary bladder. This office it serves during the whole period of the persistence of the Wolffian bodies, or until the involution of its neck with the intestine changes the anatomical relations of its ducts. Its subsequent function, however, is different, and of a more important character.

In the Mammalian Vertebrata, the embryo forms vascular and nutritive connections with the mother at so early a period that the new being exists but for a little time under independent conditions. As soon as there is direct vascular connection by means of the chorion, the independent life of the embryo ceases, and its nutrition, respiration, and other necessary functions, are performed by the mother.

But until this period, the allantois exercises a most important function, namely, that of respiration. Its surface is covered with a close

network of bloodvessels, closely resembling the pulmonary structure of the lower vertebrates.

In the embryos of the ox and goat, so young that no vascular connection had taken place with the mother, I have seen the provisional blood-corpuscles (which are at first only simple epithelial cells) become oxygenated, acquiring a red color, from circulating in these vessels.

The allantois is then probably a temporary pulmonary organ ; the form of respiration being of the lowest order, and quite in character with the condition of the embryo, that is, aquatic.

While performing this function it extends to the chorion, blends with its membranes, and its vessels pass over to it (the chorion). In this way the independent relations of the embryo cease, and the allantois as a distinct organ entirely disappears.

In the Oviparous Vertebrata, the embryonic conditions are different. Of these the Birds and true Reptiles alone have an allantois and amnion. Here the functional importance of the allantois appears greater than in the division just described.

Undoubtedly it serves here, as in Mammalia, as a urinary bladder during its earliest conditions. But its respiratory function soon appears prominent. It increases rapidly, and ultimately envelops the embryo, yolk-sac, and amnion. With these relations it performs the functions of respiration by two methods ; first, by the means of the oxygen of the liquid in its membranes ; and second, by bringing a dense network of vessels in contact with the air, which passes through the pores of the shell and surrounds the whole formation. In the latter stages of the embryonic development, it is probable that this second method is most efficient, because most direct.

From these facts we may conclude that the allantois is, *anatomically*, an appendix of the Wolffian bodies, and not of the intestinal canal, as has been supposed ; that its subsequent connection with the intestine is produced by an involution of the membranes of this last around the peduncle of the former. But whether this connection is ever a direct and tubular one, I have been unable to determine.

Physiologically it is at first the receptacle of the urinary secretion of the Wolffian bodies ; but afterwards and ultimately it is a respiratory organ.

These conclusions I have arrived at from direct studies, and it will

now be interesting to see how they agree with the general facts of the embryonic development of Vertebrata.

It is evident, that, if the allantois is an appendix of the Wolffian bodies, it would be expected to be met with only in those classes where these bodies are found. In other words, wherever we find an allantois, there ought we to find Wolffian bodies, and *vice versa.*

These relations, I believe, are true. Thus in Mammals, Birds, and the true Reptiles, we find invariably Wolffian bodies and an allantois. While in the lower Oviparous Vertebrata, as in the Amphibia and Fishes, there are neither Wolffian bodies nor an allantois.*

Thus it would appear that the views here advanced of the origin and nature of the allantois, are supported by the general embryological relations of all the classes of Vertebrata.

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3. Researches on the Development of the Viviparous Aphides. By Dr. W. I. Burnett, of Boston.†

Every naturalist is aware of the remarkable phenomena connected with the viviparous reproduction of Aphides, or plant-lice, for their singularity has led them to be recounted in works other than those of natural science, and from the days of the earlier observers they have been the theme of a kind of wonder-story in zoölogy and physiology.

I need not here go over the historical relations of this subject.

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* A remark is here necessary concerning the reputed Wolffian bodies of Amphibia. As is well known, these bodies were first described by Müller more than twenty years since. According to his own description, they differ in almost every respect from the Wolffian bodies of the higher classes.

After much examination during this last summer, I have failed to recognize in their structure and general relations the characteristics of the Wolffian bodies, and have therefore ventured to rank the Amphibia, in this respect, with the Fishes. But for further remarks on this subject, see my paper on the Development of the Renal Organs, &c., in this volume.

† This paper was illustrated by carefully made microscopic drawings, illustrating the gemmiparous development of Aphides.

The queer experiments and the amusing writings of the old entomologists are well known. The brief history of the general conditions of the development of these insects is as follows. In the early autumn the colonies of plant-lice are composed of both male and female individuals; these pair, the males then die, and the females soon begin to deposit their eggs, after which they die also. Early in the ensuing spring, as soon as the sap begins to flow, these eggs are hatched, and the young lice immediately begin to pump up sap from the tender leaves and shoots, increase rapidly in size, and in a short time come to maturity. In this state it is found that the whole brood, without a single exception, consists solely of females, or rather, and more properly, of individuals which are capable of reproducing their kind. This reproduction takes place by a viviparous generation, there being formed in the individuals in question young lice, which, when capable of entering upon individual life, escape from their progenitor, and form a new and greatly increased colony. This second generation pursues the same course as the first, the individuals of which it is composed being, like those of the first, sexless, or at least without any trace of the male sex throughout. These same conditions are then repeated, and so on almost indefinitely, experiments having shown that this power of reproduction under such circumstances may be exercised, according to Bonnet,* at least through nine generations; while Duvau † obtained thus eleven generations in seven months, his experiments being curtailed at this stage, not by a failure of the reproductive power, but by the approach of winter, which killed his specimens; and Kyber ‡ even observed that a colony of *Aphis dianthi*, which had been brought into a constantly heated room, continued to propagate for four years in this manner, without the intervention of males, — and even in this instance it remains to be proved how much longer these phenomena might have been continued.

The singularity of these results led to much incredulity as to their authenticity, and on this account the experiments were often and carefully repeated; and there can be now no doubt that the virgin *Aphis* reproduces her kind, — a phenomenon which may be continued

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* Traité d'Insectologie, ou Observations sur les Pucerons, 1745.

† Mém. du Mus. d'Hist. Nat., XIII. p. 126.

‡ Germar's Magaz. d. Entomol., 1812.

almost indefinitely, ending finally in the appearance of individuals of distinct male and female sex, which lay the foundation of new colonies in the manner just described.*

The question arises, What interpretation is to be put upon these almost anomalous phenomena? Many explanations have been offered by various naturalists and physiologists, but most of them have been as unsatisfactory as they are forced, and were admissible only by the acceptance in physiology of quite new features.

As the criticism I intend to offer upon some of these opinions will be the better understood after the detail of my own researches, I will reserve their future notice until the concluding part of this paper.

My observations were made upon one of the largest species of *Aphis* with which I am acquainted, the *Aphis Caryæ* of Harris.† While in Georgia, this last spring, it was my good fortune that myriads of these destroyers appeared on a hickory which grew near the house in which I lived. The number of broods on this tree did not exceed three, for with the third series their numbers were so great that their source of subsistence failed, and they gradually disappeared, from starvation. The individuals of each brood were, throughout, of the producing kind, no males having been found, upon the closest search; they were all, moreover, winged; and those few which were seen without these appendages, appeared to have lost them by accident. I mention this fact especially, since it has been supposed by naturalists that the females were always wingless, and therefore that the winged individuals, or the males, appeared only in the autumn.‡

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* For details of experiments by which Bonnet's original results were verified, see Réaumur, Mémoires, III. Mem. 9 and 11, and VI. Mem. 13. Also, Degeer, Mémoires, III. ch. 2, 3. Curtis, Trans. Linn. Soc., VI.; Philos. Trans., 1771. Sauvages, Journ. de Physique, I. Dutrochet, Mémoires, II. p. 442. See also the more modern writers, and especially Kirby and Spence, Introduction to Entomology, IV. p. 161.

† A Treatise on some of the Insects of New England which are injurious to Vegetation, 2d ed., 1852, p. 208. As Dr. Harris says, it is probably *Lachnus* of Illiger (*Cinara* of Curtis).

‡ See Westwood, An Introduction to the Modern Classification of Insects, &c. London, 1839, II. p. 438; — but especially Owen, Parthenogenesis, &c., p. 23, note, and p. 59, note, where he says, "Many of the virgin viviparous Aphides acquire wings, but never perfect the generative organs!"

This first brood, upon their appearance from their winter hiding-places, were of mature size, and I found in them the developing germs of the second brood, quite far advanced. On this account, it was the embryology of the third series or brood alone that I was able to trace in these observations.

A few days after the appearance of these insects, the individuals of the second brood (B), still within their parents (A), had reached two thirds of their mature size. At this time the arches of the segments of the embryo had begun to close on the back, and the various external appendages of the insect to appear prominently; the alimentary canal had been more or less completely formed, although distinct abdominal organs of any kind belonging to the digestive system were not very prominent. At this period, and while the individuals of generation (B) are not only in the abdomen of their parent (A), but are also enclosed each in its primitive egg-like capsule, — at this time, I repeat, appear the first traces of the germs of the third brood (C).

These first traces consist of small egg-like bodies, arranged two, three, or four in a row, and attached in the abdomen at the locality where the ovaries are situated in the oviparous forms of these animals.

These egg-like bodies consisted either of single nucleated cells, of $\frac{1}{3000}$ of an inch in diameter, or a small number of such cells enclosed in a simple sac. These are the germs of the third generation; they increase with the development of the embryo in which they have been formed, and this increase of size takes place, not by a segmentation of the primitive cells, but by the endogenous formation of new cells. After this increase has gone on for a certain time, these egg-like bodies appear like little oval bags of cells, — all these component cells being of the same size and shape, there being no cell which is larger and more prominent than the rest, and which could be comparable to a germinative vesicle. While these germs are thus constituted, the formation of new ones is continually taking place. This occurs by a kind of constriction process of the first germs, one of their ends being pinched off, as it were; and in this way what was a single sac is changed into two, which are attached in a moniliform manner. The new germ thus formed may consist of even a single cell only, as I have often seen, but it (the germ) soon obtains a more uniform size by the endogenous formation of new cells within the sac by which it is enclosed. In this way the germs are multiplied to a considerable

number, the nutritive material for their growth being apparently a fatty liquid with which they are bathed, contained in the abdomen, and which is thence derived from the abdomen of the first parent.

When these germs have reached the size of $\frac{1}{300}$ of an inch in diameter, there appears on each, near one end, a yellowish, vitellus-looking mass or spot, which is composed of large, yellowish cells, which in size and general aspect are different from those constituting the germ proper. This yellow mass increases *pari passu* with the germ, and at last lies like a cloud over and concealing one of its poles. I would also insist on the point, that it does not extend itself gradually over the whole germ-mass, and is therefore quite unlike a true germinative vesicle, or a proligerous disc. When the egg-like germs have attained the size of $\frac{1}{150}$ of an inch, there distinctly appears the sketching or marking out of the future animal. This sketching consists at first of delicately marked retreatings of the cells here and there, but which soon become more prominent from furrows; and at last the whole form of the embryo stands boldly out. As the whole idea and form of the insect is thus moulded out of a mass of cells, it is evident that the separate parts which then appear, such as the arches of the segments, the extremities, and the oval apparatus, consist at first of only rows of simple cells. This point is here beautifully prominent, and nowhere have I observed finer illustrations of the cell-constitution of developing forms.

The development thus proceeding, each part of the dermo-skeleton becomes more and more distinct, and the increase of size of the whole is attained by the constant development of new cells. During this time, the yellow vitellus-looking mass, situated at one of the poles of the embryo, has not changed its place; it has increased somewhat in size, but otherwise appears the same. When the development has proceeded somewhat further, and the embryo is pretty well formed, the arches of the segments, which have hitherto remained gapingly open, appear to close together on the back, thereby enclosing this vitellus-looking mass within the abdominal cavity.

It is this same vitelloid mass thus enclosed that furnishes the nutritive material for the development of new germs, which would be those of the fourth brood, or D; this development of germs here commences with the closing up of the abdominal cavity, and the same processes which we have just described are again repeated.

The details of the development subsequent to this point are like those of the development of ordinary insects, or of the Articulata in general; and although this ovoid germ has at no time the structural peculiarities of a true ovum, — such as a real vitellus, a germinative vesicle and germinative dot, — yet, if we allow a little latitude in our comparison, and regard the vitellus-looking mass as the *mucous*, and the germ-mass proper as the *serous* fold of the germinating tissue, as in true eggs, — if, I repeat, we can admit this comparison of parts, then the analogy of development between these germs and true eggs of insects may be traced in considerable detail.

This comparison I have been inclined to admit, at least in part, from the striking resemblance of these developing forms at certain stages to the embryological forms of spiders as they have been figured by Herold,* and as I have myself traced them. When, in spiders, the serous fold of the germinating tissue has extended so as to cover two thirds of the developing form, leaving the vitelline mass on the dorsal surface near one of the poles, the whole embryo quite resembles that of a developing Aphis just before the arches of the segments close up on the back.

With this view of the relative parts of the germ, the following would be the details of the development of the different systems, and in the noticing of which I shall follow Kölliker.†

1. The germinating tissue consists of two parts: a serous and mucous fold.

2. The abdominal plates arise from the serous fold, sprout out towards the vitelloid mass, pass over it, and unite on the dorsal surface of the future animal; on the opposite side are formed plates which do not unite, but are formed into the hind legs.

3. The wings are the lateral limbs.

4. The first traces of the abdominal column appear in the chain of abdominal muscles, situated between the nerves and the intestinal canal.

5. The nervous system in all its parts arises from the serous fold, as well also as the organs of sense.

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* De Generatione Aranearum in Ovo. Marbourg, 1824.

† Observationes de Prima Insectorum Genesi adjecta Articulatorum Evolutionis cum Vertebratorum Comparatione. Diss. Inaug. Scr. Alb. Kölliker. Turin, 1812. A work replete with facts and interesting suggestions.

6. The mucous fold, or the vitellus-looking mass, serves no purpose in the formation until the closing in of the visceral plates.

7. Thus enclosed in the abdominal cavity, it is not transformed directly into the intestinal canal, but simply furnishes the material from which the component cells of said canal, and its hepatic diverticula, are formed. It also furnishes the material from which the new germs are formed, as already shown.

8. The heart is formed on the dorsal aspect, between the mucous and serous folds.

In this way the details of development closely correspond with those of the embryology of the other Articulata which I have studied; and the subject is all the more interesting, as the germ-masses, from which such development occurs, in no way and at no time structurally resemble true eggs.

When the embryo is ready to burst from its developing capsule, and make its escape from the abdomen of its parent, it is about $\frac{1}{16}$ of an inch in length, or more than eight times the size of the germ at the time when the first traces of development were seen. From this it is evident that, even admitting that these germ-masses are true eggs, the conditions of development are quite different from those of the truly viviparous animals; such as, for instance, in *Musca*, *Anthomyia*, *Sarcophaga*, *Tachina*, *Dexia*, *Miltogramma*, and others among dipterous insects,* or in the viviparous reptiles; for in all these cases of ordinary viviparity the egg is simply hatched in the body instead of out of it. The egg, moreover, is formed exactly in the same way as though it was to be deposited, and its vitellus contains all the nutritive material required for the development of the egg until the coming forth of the new individual. The abdomen of the mother serves only as a proper nidus or incubatory pouch for its full development. This is true of all the ovi-viviparous animals whatsoever.† With the viviparous Aphides, on the contrary, the developing germ derives its nutritive material from the fatty liquid in which it is

* See Siebold in Froriep's Neue Notiz., III. p. 387, and in Wiegmann's Arch., 1838, I. p. 197; also his Observat. quaed. Entom., &c., p. 18.

† It is true that in the Scorpionidæ the eggs are developed in the ovary, but there is no reason to suppose that the conditions are here different from those of the viviparous Diptera. In *Oribates,* also, the eggs are developed in a kind of uterus situated directly above the ovipositor; but this appears to be only an incubatory pouch.

bathed, and which fills the abdomen of the parent.* The conditions of development here, therefore, are more like those in Mammalia, and the whole animal may, in one sense, be regarded as an individualized uterus filled with germs ; for the digestive canal, with its appendages, seems to serve only as a kind of laboratory for the conversion of the succulent fluids which the animal extracts from the tree on which it lives, into this fatty liquid, from which the increase and development of the germs take place.

When the young animal has reached its full development as an embryo, it bursts from its encasement, and appears to escape from the abdomen of its parent through a small opening (*porus genitalis*) situated just above the anus. In the species under consideration, it generally remains clinging on the back of the parent until its external parts are dry, and it is able to begin life for itself. Each parent here produces from eight to twelve individuals, and if this rapid increase is continued undisturbed, through seven to nine broods, we cannot wonder at the countless numbers which appear from so few original individuals.†

Such are the details of the embryological development of the so-called viviparous Aphides, as far as I have enjoyed opportunities for their study. We will now refer for a moment to the special points which have here been made out. In the first place, it is evident that *the germs which develop these forms are not true eggs.* They have none of the structural characteristics of eggs, such as a vitellus, a germinative vesicle and dot ; on the other hand, they are, at first, simple collections, in oval masses, of nucleated cells. Then, again, they receive no special fecundating power from the male, as is the necessary preliminary condition of all true eggs ; and, furthermore,

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* This fatty matter forms beautiful crystals of margarine, and the crystallization may well be seen to take place. The forms exactly resemble those given by Robin and Verdeil, Traité de Chim. Anat. et Physiol., Paris, 1853, Pl. XXXVIII. fig. 2 *h*.

† Réaumur has shown that in these animals the rate of increase is so great, that in five generations or broods only one Aphis may be the progenitor of five billion nine hundred and four million nine hundred thousand (5,904,900,000) descendants ; we may well ask what would be the number of descendants where the broods were extended to eleven ! ! See Kirby and Spence, Introduction to Entomology, I. p. 175.

the appearance of the new individual is not preceded by the phenomena of segmentation, as also is the case with all true eggs. Therefore their primitive formation, their development, and the preparatory changes they undergo for the evolution of the new individual, are all different from those of real ova.*

Another point is, *these viviparous individuals have no proper ovaries and oviducts.* Distinct organs of this kind I have never been able to make out. The germs are situated in moniliform rows, like the successive joints of confervoid plants, and are not enclosed in a special tube. These rows of germs commence each by a single germ-mass, which sprouts from the inner surface of the animal, and which increases in length and in the number of its component parts from the successive formation of new germs by a constriction-process, as already mentioned. Moreover, these rows of germs, which, at one period, closely resemble in general form the ovaries of some true insects, are not continuous with any uterine or other female organ, and therefore do not at all communicate directly with the external world. On the other hand, they are simply attached to the inner surface of the animal, and their component germs are detached into the abdominal cavity as fast as they are developed, and then escape outwards through a *porus genitalis*, exactly as is the case with the eggs of fishes.† Here, then, comes the important question, What interpretation shall be put upon these reproductive parts, — these moniliform rows of germs? Ignoring all existing special theories relating to reproduction, the observing physiologist would be left no alternative but to regard them as *buds*, true gemmæ, which sprout from the inner surface of the Aphis, exactly like the buds from the external skin of a Polyp.‡

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* Milne-Edwards thinks he has found true *ova* and ovaries in the viviparous forms of these animals. (Quoted by Dr. Carpenter in Brit. and For. Med. Chir. Rev., 1849, IV. p. 443.) I think he must have been deceived, as I was at first, by the general appearances which, unless carefully examined, closely resemble those of true oviparous individuals.

† These observations of mine on the special anatomy of the reproductive parts of viviparous Aphides agree with those of Siebold, who studied the subject with much care several years since. See Froriep's Neue Not., XII. p. 308. Siebold, however, regarded them as true ovaries and oviducts, but without any of the usual appendages which are found in the true oviparous Aphides.

‡ I would insist upon this wide and important distinction between buds and

Before proceeding to a discussion of the relations of this important conclusion to which we have just arrived, it may be well to refer to the views of others upon the exact signification of these singular reproductive phenomena.

Those old entomologists, such as Bonnet, Réaumur, Degeer, &c., who were the first to observe, besides verifying beyond all doubt, these peculiar phenomena, all believed that each brood constitutes a separate generation, and that the reproduction takes place by true ova, as in the common generative act of other insects. This wide deviation from the ordinary course of nature, as it seemed to them, they attempted to explain and reconcile by various theories. Thus, Réaumur* affirmed that these viviparous individuals were androgynous; and, in later times, Leon Dufour,† who knew too well the anatomical structure of insects to believe with Réaumur that they could be hermaphrodites, referred these phenomena to spontaneous or equivocal generation. Morrem,‡ who made somewhat extended researches on the anatomy of *Aphis persicæ*, and especially of its generative organs, advanced the novel theory, that these broods were developed in the body of the virgin parent, by a previously organized tissue becoming individualized and assuming an independent life, exactly as he believed to be the case with Entozoa. To each and all of these views, it scarcely need be said that they would be wholly inadmissible according to the present established doctrines of physiological science, even had we no directly controverting observations.

But there are other explanations or views which deserve more attention. The first of these is that advanced by Kirby and Spence.§ According to them, "One conjunction of the sexes suffices for the impregnation of all the females that in a succession of generations spring from that union." In support of the reasonableness of this hypothesis, they quote several instances which they regard as of analogous character; thus they say in regard to the hive-bee, that "a single intercourse with the male fertilizes all the eggs that are laid for the space of two years."

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ova. The structure and conditions of all ova are the same, and there is no passage between them and buds. But this point will be enlarged upon hereafter.

* Mémoires, *loc. cit.*

† Recherches Anat. et Physiolog. sur les Hemiptères, Paris, 1833.

‡ Anat. de l'Aphis Persicæ, in the Ann. des Sci. Nat., V., 1836, p. 90.

§ Introduction to Entomology, IV. p. 161.

In this connection should be mentioned the similar hypothesis advanced for a like purpose by Jourdan.* According to him, many Lepidoptera lay fertile eggs when completely isolated from the males. Such are *Euprepsia casta*, *Episema cœruleocephala*, *Gastropacha potatoria*, *G. quercifolia*, and *G. pini*, *Sphinx ligustri*, *Smerinthus populi*, and *Bombyx querci*.

But all these cases have really no strict analogy with that of the Aphides in question; for there is not, as with these last, a succession of innately fertile individuals, but only females which are capable of producing several broods from a single coitus, or after having been long removed from the males, which may even then be dead.† Late researches upon the minute anatomy of the generative organs of insects have furnished results by which these phenomena, seemingly strange at first, can be explained. All these insects, which are thus capable of laying fecundated eggs again and again after the first impregnation, have a *receptaculum seminis* connecting with the oviduct, in which the semen is deposited during coition, and where it may be preserved without losing its vitalizing power for several months.‡ Thus, by this provision, the males, having copulated with the females

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* Manuel de Physiologie, par J. Müller, trad. de l'Allemand, etc., par A. J. L. Jourdan, deux ed. rev. et annot., by E. Littre, II. p. 599, note.

† Siebold has made observations upon allied phenomena occurring in the Psychidæ, which are of no little interest. He has shown that in the genera *Psyche* and *Fumea*, the alleged reproduction, *sine lucina*, is unfounded, — these insects having well-formed internal genital organs, and the male being adapted to impregnate the female while the latter is in her case. But in the genus *Talœporia*, Siebold has shown that there is propagation *sine concubitu*, exactly as occurs with the Aphides. See Ueber die Fortpflanzung der Psyche: Ein Beitrag zur Naturgeschichte der Schmetterlinge, in Siebold and Kölliker's Zeitsch., I. 1849, p. 93; but for his last researches on *Talœporia*, see his Bericht üb. d. entomol. Arbeiten d. schles. Gesellsch. im J. 1850, — or its English transl. in the Trans. of the Ent. Soc., N. S., I. p. 234.

‡ For many details on this subject of the *receptaculum seminis*, see Siebold, Müller's Arch., 1837, p. 392; also in Weigmann's Arch., 1839, I. p. 107, (*Vespa*,) and in Germar's Zeitsch., II., 1840, p. 442 (*Culex*). See also Stein, Vergleich. Anat., &c., 1847, pp. 96, 112.

I cannot but believe that the anomalous reproductive conditions of the Cynipidæ will, at last, have a solution equally satisfactory. See Hartig Germar's Zeitsch., II. p. 178, and IV. p. 395. See also Siebold and Stannius, Comparative Anatomy, transl., I. § 348, notes 1 and 4.

in the autumn, may immediately die, while these last, hibernating, produce in the spring fertile ova; and in the instance of the *Bombus Americana*, such a coition suffices for all the three broods which are produced the ensuing summer.

Another explanation of these curious phenomena, and which has attracted some attention, as well from its singularity as from the eminence of its propounder, is that of Owen, advanced in his Hunterian Lectures in 1843.*

He affirms that the larval Aphides are productive in virtue of the successive continuation from brood to brood of a portion of the primitively fertilized germ, and which material product or leaven is not exhausted until nine to eleven generations. I will quote his own language: "In the Aphides the corresponding vitelline cells retain their share of the fecundating principle (which was diffused through the parent egg by the alternating fissiparous, liquefactive, and assimilative processes) in so potent a degree, that a certain growth and nutritive vigor in the insect suffice to set on foot, in the ovarian, nucleated cells, a repetition of the fissiparous and assimilative process, by which they transform themselves in their turn into productive insects; and the fecundating force is not exhausted by such successive subdivision until a seventh, ninth, or eleventh generation." This same doctrine,—the successive inheritance of a portion of the primary germ-mass from brood to brood, and by means of which the fertile germs are continued,—this doctrine, I say, is repeated in full in this author's work on Parthenogenesis, and I will here quote one sentence, not only in illustration of this, but to show how different his own observations on the development of these animals are from mine, just described. He says: "One sees such portion of the germ-mass taken into the semi-transparent body of the embryo Aphis, like the remnant of the yolk in the chick. I at first thought it was about to be enclosed in the alimentary canal, but it was not so. As the embryo grows, it assumes the position of the ovarium, and becomes divided into oval masses and enclosed by the filamentary extremities of the eight ovi-

* Lectures on the Comparative Anatomy and Physiology of the Invertebrate Animals, &c., London, 1843, p. 233. This explanation is lately insisted upon (strange to relate) in his recent work, "On Parthenogenesis, or the Successive Production of Procreating Individuals from a single Ovum," London, 1849.

ducts. Individual development is checked and arrested at the apterous larval condition. It is plain, therefore, that the essential condition of the development of another embryo in this larva is the retention of part of the progeny of the primary impregnated germ-cell." (p. 70.)

This view of Owen, so ingenuously advanced, and which he has made subservient for the chief support of his new doctrine of Parthenogenesis, is indeed plausible, and seems at first satisfactory: but, as I hope to show, it will not bear analysis.

In the first place, it is evident that Owen does not recognize any physiological difference between a *bud* and an *ovum*. This is clear from what he remarks in the first quotation; but in his work on Parthenogenesis he has said so in as many words. "The growth by cell-multiplication producing a bud, instead of being altogether distinct from the growth by cell-multiplication in an egg, is essentially the same kind of growth or developmental process." (p. 45.)

Here is a fundamental error, which, if not removed, will obscure all our views of the physiology of reproduction. I have already insisted upon the necessity of this broad distinction between these two forms, a necessity based not only upon differences of anatomical constitution, but also upon physiological signification.

An *Ovum* is the exclusive product of an individual of the female sex, and is always formed in a special organ called the ovary. It is the particular potential representative of the female, and has its ulterior development only from its conjunction with a corresponding element of the opposite or male sex; and zoölogy presents no instance where there is development from eggs, unless these conditions of the two sexes are fully carried out.

A *Bud*, on the other hand, is simply an offshoot from the form on which it rests,—a portion of the animal capable of individual development. It sustains, therefore, no relations to sex, and, in truth, is widely separated in its ulterior signification from that cycle of processes conceived in a true oviparous reproduction.

All physiologists who have carefully studied embryological and developmental processes, must feel the correctness and importance of this distinction, which lies in realities and not in words.

It is true that a bud and an ovum are composed each of the same elements,—simple nucleated cells; but in one, these cells are simply in a mass, while in the other, they have, throughout the animal king-

dom, high or low, a definite and invariable arrangement. Then, again, as to the constitution of each and both being, on the whole, of nucleated cells, it may be said, that it could hardly be conceived to be otherwise, for nucleated cells are the elementary components of all functional organized forms; and it may be added, moreover, that he knows little of the highest physiology, who has not learned that widely different teleological significations may be concealed beneath isomorphic animal forms.

I have thus dwelt rather at length upon this point, because I think it is a vital one in our subject, and the possession of clear ideas thereon will be found singularly conducive to our correct appreciation of this whole class of anomalous phenomena under discussion. But we will revert to the subject of Owen's hypothesis.

As to the chief point in this hypothesis, the continuation of the primary germ-mass as a leaven, from brood to brood, it requires but little thought to perceive that it is physically impossible. I would first allude to Owen's statement, quoted above, that a portion of the germ-mass is taken into the abdomen of the embryo Aphis, and, as he thinks, assumes, without any change, the position of the ovarium. By this he refers, undoubtedly, to the vitellus-looking mass I have described in my observations, and according to which, also, it appeared to serve only as the nutritive material out of which the digestive organs and the germs are formed. Moreover, I feel quite sure that the germ-cells are new cells formed in the abdomen, and not those derived from the parent.

But the point I wish to enforce is, that, even admitting that individuals B may contain an *actual residue* of individuals A, it is clearly evident that this succession must stop with brood B; for these residual germ-cells which compose B in its earliest conditions are lost in the developmental processes, and the germs of individuals C, which are found in B, are each primarily nucleated cells formed *de novo*, as I have observed and above described. With these observed conditions of development, it is impossible for the individuals of the successive broods to inherit the original spermatic force in the continuation of the original cells.

The hypothesis of Owen, therefore, plausible and ingenious as it may seem, does not appear to me to accord either with observed facts, or with the soundest physiology of the reproductive processes.

I may here remark, also, that this doctrine of Parthenogenesis, based as it is upon the conditions of the hypothesis in question, cannot, as such, be sustained, for the same reasons; and all its phenomena would appear to find their solution either in Steenstrup's doctrine of "Alternation of Generations," so called, or in the conditions of true gemmiparity, — admitting, provisionally, that Steenstrup's doctrine and gemmiparity include really different physiological conditions.

But the most important explanation advanced, and the last which I shall notice, is that offered by Steenstrup* in his doctrine of the "Alternation of Generations," and of which it forms a chief support. The details of this peculiar doctrine of Steenstrup I need not here furnish; they are well known to all physiological anatomists. Its features, however, may be expressed in a formula-like manner. Individuals A produce true fecundated eggs, from which are hatched individuals B, which are unlike their parents in all zoölogical respects, but in which are developed spontaneously, and without any reference to sex, germs which ultimately become individuals like A, and so the cycle of development is completed. These intermediate individuals, B, Steenstrup has termed nurses (*Ammen*), and he regards them as distinct animals, subservient for a special end; he therefore considers that B constitutes a real generation.

Instances of such phenomena are found in the lower orders of the animal kingdom, — Polyps, Acalephs, and Worms; and late research has shown that they are more or less common throughout the whole of the Invertebrata.

The difference between alternation of generation and metamorphosis is too marked to require illustration; in the latter there is the same individual throughout, and the developmental processes, although concealed beneath different exteriors, are regular and normal; with the former, however, this chain of development is broken by one form being developed in another, this intermediate form serving as a stepping-stone for a higher and ulterior development. Another important point in this alternate reproduction is, that in each new change some

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* On the Alternation of Generations, or the Propagation and Development of Animals through Alternate Generations, a Peculiar Form of Fostering the Young in the Lower Classes of Animals, transl. by the Ray Society, London, 1845, *passim*.

real progress is made, — the nursing form being manifestly inferior to the individual which gives it rise.

Steenstrup regards the Aphides as furnishing the most perfect examples known of nursing individuals, and, on the whole, as constituting typical illustrations of this doctrine he has advanced.*

But if this doctrine implies conditions other than those which belong to true gemmiparity, it does not appear to me that it has any support in the phenomena in question of the Aphides. And although I am inclined to believe, as I shall soon show, that all these phenomena, essentially, may be of the same nature, yet there can be no doubt that the manifestations are here somewhat peculiar. With the Aphides there is no real morphological progress made in each brood, for the viviparous individuals are, zoölogically, as perfect in every way as those which are oviparous, except in their want of true sexual generative organs. I have shown that, in the one species here described, they had well-developed wings like the true sexual individuals. Moreover, each brood, from the first to the last, inclusive, is a repetition of the same. But these conditions are external and economical, and, instead of offering these prominent examples as evidence against the validity of Steenstrup's doctrine, I would rather present them as broadly indicating that, after all, this doctrine in question involves no conditions excepting those belonging to a modified form of gemmiparity. All the instances of Polyps, Acalephs, Worms, Insects, &c. would then be classed in the same category, and the variations in manifestation would belong rather to the economical relations of the animal, than to any intrinsic difference of physiological process. Thus, the Distoma nurses, instead of being developed to a condition at all resembling their parent, remain persistent on a low form; and not only is their whole zoölogical character undeveloped, but they also experience morphological changes from the developmental process, which immediately go on within them. All this is in perfect keeping with their economy as animals, for the low order of their conditions of life does not necessitate a higher and more truly zoölogical form of these nurses from which are to be developed the true animals; were it otherwise, I cannot but believe that both the nurses and the grand-nurse of Distoma would quite resemble the original

* See *loc. cit.*, p. 112.

animals. In the case of the Aphides, the economical conditions are different, and finely illustrate this point.

The Aphis nurse, in virtue of its very typical structure as an insect, must live under higher conditions, and so its development, zoölogically, proceeds to a corresponding point; this point is where it, as an insect and as an Aphis, can furnish the nutritive material for the development of its endogenous germs.

Herein, then, would appear to consist the prominent morphological differences observed in this category of phenomena, and I need not labor further to show that they are irrelevant to the primary essential conditions of these curious processes.

Such appears to me to be the highest, both physiological and zoölogical, interpretation that can be advanced for these phenomena which Steenstrup has so ingeniously collected and collated; and to advance the view that these intermediate individuals or nurses are not intrinsically and zoölogically the same as their parents, but furnish examples of how dissimilar animals may arise from a common stock, — to put forth this view, I say, is to advocate a doctrine in physiology as mischievous as it is deeply erroneous. I think, therefore, that the doctrine of Steenstrup may prove to be unfounded, as far as it would involve, intrinsically, new phenomena in the process of reproduction; and, as I have said on a preceding page, all its conditions may find their illustration and solution in the various phases of gemmiparity.*

If in this discussion of some of the highest relations of physiology we have not wandered too far from our subject proper, which we have thereby sought to illustrate indirectly, we will revert to the thread of its discourse for a few concluding remarks.

The final question now is, What is the legitimate interpretation to be put upon the reproductive phenomena of the Aphides we have described? My answer to this has been anticipated in the foregoing

* This statement is made perhaps more strongly and exclusively than the present state of our knowledge would warrant, but I throw it out much in a suggestive way. There is no subject in physiology more interesting and comprehensive than that of *Gemmation;* the important question now is, Does it, as an individual process, embrace all the categories of phenomena treated by Lovén, Steenstrup, &c., these phenomena varying extrinsically, according to economical conditions, or do they (the phenomena) imply something beyond and dissimilar to gemmation?

remarks. I regard the whole as constituting only a rather anomalous form of gemmiparity. As already shown, the viviparous Aphides are sexless; they are not females, for they have no proper female organs, no ovaries nor oviducts. These viviparous individuals, therefore, are simply gemmiparous, and the budding is here internal, instead of external, as in the Polyps and Acalephs; it, moreover, takes on some of the morphological peculiarities of oviparity; but all these dissimilar conditions are economical and extrinsic, and do not touch the intrinsic nature of the processes concerned therein.

Viewed in this way, the different broods of Aphides cannot be said to constitute as many true generations, any more than the different branches of a tree can be said to constitute as many trees; on the other hand, the whole suit, from the first to the last, constitutes but a single true generation. I would insist upon this point, as illustrative of the distinction to be drawn between *sexual* and *gemmiparous* reproduction. Morphologically, they have, it is true, many points of close resemblance; but there is a grand physiological difference, the true perception of which is deeply connected with our highest appreciation of individual animal life.* A true generation must be regarded as resulting only from the conjugation of two opposite sexes, — from a sexual process in which the potential representations of two individuals are united for the elimination of one germ. This germ-power may be extended by gemmation or by fission, but it can be formed only by the act of generation, and its play of extension and prolongation by *budding*, or by division, must always be within a certain cycle, and this cycle is recommenced by the new act of the conjugation again of the sexes.

In this way the dignity of the ovum as the primordium of all true individuality is maintained; and the axiom of Harvey, *Omne vivum ex ovo*, stands as golden in physiology. The buds may put on the dress and the forms of the ovum, but these resemblances are extrinsic, and in fact only an inheritance from their great predecessor.

These phenomena thus interpreted furnish us an excellent key to

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* In this view, as well as in several others herein discussed, I am pleased to say that I have the support of so learned a physiologist as Dr. Carpenter. See his Review "On the Development and Metamorphoses of Zoöphytes," in the Brit. and Foreign Med. Chir. Rev., 1848, I. p. 183; and on "Reproduction and Repair," in Ibid., 1849, II. p. 419.

many others which have long been regarded as anomalous in the history of development.

I refer here to the so-called hibernating eggs (*Wintereier*), which are found in many Invertebrates. These I have not seen, but they have been carefully described by several trustworthy observers. These so-called eggs consist of oval masses or cells, invested with a capsule, but in which no germinative vesicle and dot have ever been seen. Structurally, therefore, they do not resemble eggs, and it is from their form and ulterior development only that they have received this name. Moreover, they sustain none of the usual relations of eggs to the sexual organs, and, as far as I am aware, no one has witnessed their development in the ovaries. These bodies have been observed in *Hydatina** and *Notommata*† among the Infusoria; in *Lacinularia*‡ among the Rotatoria; and in *Daphnia*§ among the Crustacea. In all these instances they hatch without the aid of the male, the existence of which sex was once doubted from its infrequent appearance.

Now I regard these hibernating eggs as merely egg-like *buds*, exactly corresponding to the germs of the viviparous Aphides. In other words, there are in the animals I have just mentioned certain individuals which reproduce by buds, which are developed under rather anomalous conditions; and I will add, in conclusion, that I suspect that this gemmiparous mode of reproduction will be found to be far from uncommon among most of the Invertebrata, when our researches into the history of their development shall have been more widely extended.||

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* Ehrenberg, Die Infusionsthierchen, p. 413.

† Dalrymple, Philos. Trans., 1849, p. 340.

‡ Huxley, Quarterly Jour. Micr. Sc., 1852, I. p. 13.

§ Müller, Entomostraca, p. 84, Tab. XI. fig. 9–11, Tab. XII. fig. 5. Also Ramdohr, Beiträge zur Naturgesch. einiger deutschen Monokulus-Arten, 1805, p. 28; Straus, Mém. sur les Daphnia, in the Mém. du Mus. d'Hist. Nat., V. p. 413, Pl. XXIX.; Jurine, Histoire des Monocles, 1820, p. 120, Pl. XI. fig. 1–4. Jurine calls these aggregated eggs "La maladie de la selle."

There is, moreover, reason to believe that these anomalous reproductive conditions occur in nearly all the Entomostraca; — see Siebold and Stannius's Comparative Anat., my transl., Vol. I., my note under § 292, note 4.

|| Notice may be here given of some curious observations which Filippi (Ann. Nat. Hist., IX., 1852, p. 461) has furnished on the development of the Pteromalidæ. A *Pteromalus* lives in the ova of *Rhynchites betuleti;* in each of these ova there is seen,

P. S. I regret that I should not have seen until now, when this paper is concluded, the important writings of Leydig on the subject under discussion. In his article "Einige Bemerkungen über die Entwickelung der Blattläuse," in Siebold and Kölliker's Zeitsch. f. wiss. Zoöl., II., 1850, p. 62, he speaks of his former observations in the Isis, 1848, III. p. 184. These I have not seen, neither also a work to which he refers, of J. Victor Carus (Zu näheren Kenntniss des Generationswechsels, Leipzig, 1849). Leydig, in his criticism of Carus's views, expresses the opinion, that the development of the viviparous Aphides is, histologically, like that of the Articulata in general. According to him, also, the germ-bodies undergo processes corresponding to those of impregnated eggs. These statements of Leydig, who is an excellent observer, have induced me recently to repeat my observations; but this afforded the same results as before, namely, that the germ-bodies out of which are developed the viviparous Aphides, have no true histological identity with eggs.

Additional Note.

Since the preparation of the paper on the development of Aphides, I have enjoyed the opportunity of making this series of investigations more complete, by an examination of the terminal or last brood, which appears at the end of autumn.

This terminal brood has hitherto been considered, as far as I am aware, to be composed exclusively of males and females, or, in other words, of perfect insects of both sexes. I was surprised, therefore, on examining the internal organs of the non-winged individuals, to find that many of these last were not females proper, but simply the

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soon after its deposit, a minute infusorial animal, with a tail by which it moves briskly about among the vitelline cells. It soon ceases to move, however, and in its interior appears a vesicle, which increases and changes into a larva, which is that of *Pteromalus;* this larva becomes a pupa, and, after eight or ten days, changes to the perfect insect, which escapes from the ovum.

If these observations are verified, we have here a case exactly like that of the Aphides, excepting that, like the *Distoma*, the intermediate budding form is very low, and takes on none of the zoölogical peculiarities of the parent. But these statements need corroboration, for they do not agree with the history of other species of *Pteromalus* whose development is well known. See also the wonderful gemmiparous phenomena related by Siebold of *Gyrodactylus*, in Siebold and Kölliker's Zeitsch. f. wiss. Zoöl., I., 1849, p. 347.

ordinary gemmiparous form already described. Moreover, so great was the similarity of appearance between these two forms, — true females and gemmiparous individuals, — that they could be distinguished only by an examination of their internal genitalia. Among the proper females there were, besides those which were filled with eggs, or had already deposited them, other individuals in which the ovaries were but feebly developed, or, at least, in which no mature eggs had been formed. An opportunity was thereby afforded me to examine the structural differences between the true ovaries and their *quasi* representatives, the bud-like processes. The true ovaries had their usual, well-known structure, — multilocular tubes containing nucleated cells, which are probably the undeveloped germs; the bud-like processes, on the other hand, consisted of a row of cell-masses, oval, and connected by a kind of peduncle, as described in detail in the preceding paper. These wide differences have, more than ever, persuaded me of the morphological dissimilarity of these two kinds of reproducing parts in this animal. It seems to me, then, that the real intrinsic difference between an ovum and a bud lies as deep as the conditions of sex itself, notwithstanding the latter often has, as in the present case, for instance, some of the morphological characteristics of the former.

The appearance of sexless, gemmiparous individuals in the terminal brood would seem to indicate, moreover, that the conditions which determine the appearance of individuals usually exclusively male and female, are not, perhaps, referable to the fact of this being the last brood, but rather to relations of warmth and nutrition. This view is rendered more probable by the fact of the variation in the number of broods between the first and last observed in the same species in different years, ranging from seven to nine, eleven, or more. Moreover, Kyber, as quoted already in the preceding paper, by nursing continually in a warm room a collection of *Aphis dianthi*, keeping about them a summer temperature, succeeded in continuing uninterruptedly the series of sexless or gemmiparous individuals for four years. There are many other facts in insect life that indicate in like manner some direct relation between temperature and nutriment and definite sexual development. The subject is as important as it is interesting in physiology, and these very animals will, perhaps, subserve the successful study of the primary morphological conditions of sex.

4. On the Blood-corpuscle-holding Cells, and their Relation to the Spleen. By Dr. W. I. Burnett, of Boston.

The history of the spleen is a remarkable one in physiological science. From the earliest times to the present day, it has been the opprobrium of investigators, and the dignified old Haller, after searching in vain for its functional relations, concluded that it was an unimportant and unworthy part in the economy. No less than fifteen theories as to its use have been advanced and defended, up to the present time; these embrace every ostensible view of its function, having any ground of plausibility. It can be easily conceived, then, that modern physiologists might conclude, when at last from chemical and microscopical research the true function of this organ should be made out, that it would be only one of the old theories revived. But this, modern ingenuity has shown, may not be the case; for quite recently a theory has been advanced on this subject, which is unique, and in no respect like any of the preceding. Kölliker, the distinguished Würzburg Professor of Anatomy and Physiology, maintains that the spleen is the blood-destroying organ. It is indeed a significant question in physiology, What becomes of all the blood-corpuscles that have been used in the system? We find no traces of their decay in the bloodvessels, and, from all the appearances in the circulation, they never pass away. After they have served their part as oxygen-carriers, where is their final resting-place? Kölliker supposes that this refuge is in the spleen, an organ in which they congregate, assume the form of globular masses, then crumble and are dissolved, passing off for a biliary or some other purpose.

This point opens for discussion several interesting topics, chief among which is the *modus operandi* by which Kölliker thinks this process is accomplished. In order to understand this, the general relations and the intimate structure of the spleen must be briefly noticed.

None of the Invertebrata possess this organ; but it may be said, in general, to be present in all the Vertebrata, for its doubtful existence is with some of the Myxinoid fishes only, and even here it may well be questioned if the general rule does not hold good. It is evident, therefore, that the Vertebrata have relations in their economy

on which the presence of this organ depends. This point, however, will be reserved for future discussion. In its gross aspect, the spleen appears as a red, pulpy organ, with white points here and there dotting its surface when its investing membrane is removed, or when its structure is exposed by an incision. This red, pulpy matter is composed chiefly of blood-vessels held in place by an intercellular or delicate parenchymatous substance, made up of nucleated cells, and of peculiar muscular fibres which are of recent description.

The white points are the Malpighian corpuscles, which consist of vesicles filled with granular, nucleated cells, not unlike those found in the general parenchyma. Such is the intimate structure of this organ with the higher Vertebrata ; but with the lower Vertebrata, it is much less complex, no Malpighian bodies being found. On the whole, the spleen is a vascular, parenchymatous organ, composed so largely of bloodvessels that it has much variability of size in the same individual, according to the flaccidity or turgidity of these vessels. We will now proceed to the points at issue in Kölliker's theory of the function of this organ.

In the splenic tissue of many animals, there are not unfrequently observed roundish or oval masses of a brownish color, and of a size varying from $\frac{1}{200}$ to $\frac{1}{1500}$ of an inch in diameter. These appear to be aggregations or collections of blood-corpuscles, which may or may not be invested with a distinct capsule. These corpuscles are in some stage of disintegration, and appear more or less brown, as this dissolution proceeds. These bodies, Kölliker thinks, occur very generally and constantly in the spleen, and he supposes that, by them, this organ serves for the working up of old, worn-out blood-corpuscles.*

I need scarcely say, that so novel a theory, and one, too, with such ostensible plausibility, has attracted the attention and provoked much discussion among physiologists. Indeed, I know of no special subject in physiology that has received more attention from some of the best microscopists than this. Remak, Ecker, Virchow, Gerlach, Reichert, Kölliker, and many others, have given it special attention, and already its literature is not insignificant. I cannot here give an analysis of the

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* This view Kölliker advanced in 1847, but it was first enunciated in a complete form in 1849, in the article *Spleen*, in the Cyclop. Anat. and Physiol. See Literature, *post*, p. 229.

somewhat different results of these observers, but it may be mentioned that, although all admit that bodies such as I have described do occur in the spleen, yet none of them, as I understand, accept Kölliker's hypothesis of their functional relations,* and all have not observed them with that frequency and constancy mentioned by Kölliker. As for myself, I have carefully examined the spleen throughout the Vertebrata, with special reference to the doctrine in question. These examinations have extended through whole tribes of fishes, reptiles, birds, and mammals. I have not found these bodies by any means as common as Kölliker has stated; indeed, they have appeared to me under circumstances which seemed more accidental than normal, and, with comparatively few exceptions, they were unenclosed by a distinct membrane. This was found to be the case especially with birds and reptiles. It is true that, with the rabbit and other rodents which I have examined, these bodies were regularly saccular when present, but they were of various sizes; and it is worthy of notice, that, in these animals, blood, taken from other parts of the body, showed a disposition to a grouping of its corpuscles into masses of variable size.

In some reptiles I have found these bodies present at one time and absent at another, and with the frog, where Kölliker says they may be beautifully seen, I have generally failed to detect them. On the whole, then, my conclusion is, that the blood-corpuscle-holding cells of Kölliker are accidental rather than normal productions, and that they sustain no relations to the function of the spleen. I regard them as simple, minute extravasations of the blood into the spleen parenchyma, which may or may not be invested with a membrane, according to the greater or less plasticity of the blood.

This view is supported by many facts which have been observed in both physiology and pathology. If inflamed blood be drawn from the body and allowed to stand, there are, not unfrequently, found at the bottom of the vessel bodies consisting apparently of little sacs filled with blood-corpuscles. This is due to the fact, that a certain number of corpuscles become collected into one mass, and this last is then invested by a membrane composed of the blood-plasma.

That this is the correct version is shown by the fact that these capsular bodies sometimes include other matters besides simple blood-

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* I would except Gerlach, Ecker, and Beclard; see Literature, *post*, p. 229.

corpuscles, and Kölliker himself mentions a case where, in an extravasation of blood into the *commissura mollis*, he found them containing pieces of cerebral substance. I would mention, also, that I have observed these conditions distinctly marked in some blood of an elephant, which was recently examined.

My opinion, finally, is like that of Remak, that these peculiar bodies are minute blood-clots (*Blutgerinnsel*), of accidental occurrence, and of no physiological significance.

It now remains for me to notice in this connection another subject, to which the one we have just treated is somewhat allied. I mean the so-called pigment-holding cells of Remak. The history and relations of these bodies have been brought out recently by the discussion of the constituents of the spleen to which we have just alluded. If the spleen of fishes is examined, there will be found on the sheath of some of its smaller arteries roundish or oval bodies, which consist of a thick-walled capsule enclosing pigment granules. These are the bodies in question; they have not unfrequently been mistaken for the Malpighian corpuscles. They are tough, and the last parts of the spleen to decay, so that they are often best seen when the contiguous parts are just beginning to disintegrate.* I have never observed them in this connection with the minute arterial sheaths, except in fishes. Here I have carefully studied them; they appear to be perfectly closed sacs, and have no communication with the artery on which they rest.

The constancy of their presence, as well as the general regularity of their size in individual species, would lead me to agree with Remak in considering them as true physiological organs. They seem to be producers of pigment granules, the ulterior use of which remains yet to be discovered. It may be that these granules are concerned in the coloring and special constituents of the bile.

The following appears to be the most probable view of the function of the spleen in the economy, as elucidated by the more modern microscopical studies.

I think there can now be but little doubt that the systematic position

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* For figures of these bodies, see Kölliker, article *Spleen,* Cyclop. Anat. and Phys., 1849. Ecker, in R. Wagner's Icones Physiol., Lfrg. 1, 1851, Taf. VI. Remak, in Müller's Arch., 1852, p. 115, Taf. V. fig. 4, 9, 10.

of the spleen is in the category of ductless glands,—belonging, as to its physiological signification, with the supra-renal capsules and the thymus and thyroid bodies. Its structure and its development both indicate the correctness of this view.

Experiments have shown that, whatever may be its function, the presence of the spleen is far from being indispensable in the body, or even necessary for its health.* The whole history of its development and formation in the embryo clearly shows that it can have no particular function during the embryonic life. In the chick, where its earliest conditions have been most carefully traced, it is found that it reaches no functional capacity † until the period of incubation is over.

Its relations to the economy, then, do not begin until the animal has commenced to have a nutrition of its own. In this respect, therefore, it is inverse to the other ductless glands; for, as has been well shown by Ecker and other observers, the thymus and thyroid bodies, and the supra-renal capsules, are truly embryonic organs, and cease to perform any function in the individual animal.

I would, therefore, say, that the whole tenor of present research upon this organ, combined with many incidental pathological phenomena, favors the view that the spleen has functional relations in the adult life, corresponding to those of the other ductless glands, just mentioned, in the embryo; that is, it is intimately connected with the formation of the red corpuscles of the blood. I would not say, with Gerlach and Schäffner,‡ that it is the locality where these corpuscles are formed; but rather, that it has more the office of a lymphatic gland, where the plastic materials of the blood are prepared and made ready for their conversion into the red corpuscles; or, to speak more to the point, it is one of the organs in which chyle-corpuscles are formed and eliminated preparatory to their being changed into, or rather serving as the basis for the formation of, the true red

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* The sensible remark of Haller is well worthy of quotation in this connection. "In utilem aliquam partem corporis animalis esse tam late per divisas species regnantem, indignum est dictu." — Elem. Physiolog., VI. 426.

† See Gray, "On the Development of the Ductless Glands in the Chick." Philos. Trans., 1852, Part II. p. 295.

‡ See Literature, next page.

corpuscles. The Malpighian vesicles are undoubtedly prominent agents in these processes, and Gray has shown that their development here is precisely like that of the vesicles in the thymus and thyroid glands. In this connection should also be mentioned the fact of the existence of the spleen in all the Vertebrata which have both red and white corpuscles in their blood, and its non-existence in all the Invertebrata, which have the white corpuscles alone. Moreover, the spleen is most prominent in those Vertebrata which have the red constituents of the blood most marked, while with the Myxinoid fishes, whose blood quite resembles that of the Articulata, the spleen is so feebly developed that its existence was for a long time denied.

These data are worthy of remembrance, and without a more detailed discussion, I think we are justified in concluding, if any conclusion is now proper, that *the spleen is a vascular-lymphatic gland, whose function is intimately connected with the formation of the blood, by the elaboration of the chyle-products.*

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5. On the Reproduction of the Toad and Frog, without the Intermediate Stage of Tadpole. By Dr. W. I. Burnett, of Boston.

From the days of the earlier naturalists, there have been observed phenomena in the reproduction of toads and frogs which were anomalous and difficult of explanation. Both Shaw and Ray, as well as many others, noticed these animals appearing under conditions in which their birth and early period of life in water seemed quite impossible. They therefore attempted to explain this peculiarity by supposing that these animals were sometimes viviparous. Quite recently the subject has attracted some attention in England, and Mr. Lowe* has stated some facts which would show that the observation of these older naturalists is correct, and that therefore toads at least may be produced without the larval tadpole state. A Mr. Jenyns† has also confirmed these views. It is well known that the common mode of reproduction with these animals is, that they repair to the water at the breeding season, where the eggs are laid, which are soon hatched into a fish-like animal, — the tadpole. This animal breathes by gills, and its whole life, as well as many parts of its structure, correspond with those of fishes. Here the extremities, the hind and fore legs, are developed, the tail is dropped, and the animal is then a frog or toad, ready and capable of terrestrial life, breathing by lungs and capable of locomotion. The whole structure of general or special parts of these animals does not admit of their viviparity, and the eggs in all instances must be fertilized out of the body. Such are the general relations of the case. Now, it is well

* Annals of Natural History, XI. 1853, p. 341.
† Annals of Natural History, XI. 1853, p. 482.

known that toads live in cellars year after year, where they re-produce, myriads of little toads appearing all at once, and where there is no water accessible. I have also observed them breeding most prolifically in gardens which were tightly boarded in, and which were removed a mile and a half from any fresh water. This spring, in fact, a gentleman's garden, near Boston, was so filled with them, that he could scarcely walk about; and a new brood which was just appearing I carefully examined. The little toads were so small that they could scarcely hop, and were evidently only a few days old. It could not be possible that they should have come from any water, the nearest of which was more than a mile off. More careful examination showed that they must have been bred in the bank wall of the house, for this was filled with holes out of which the old ones went and came. Here, therefore, these animals were produced without water, and without, of course, the tadpole, gill-breathing stage. The eggs must have been hatched in damp earth, and the little animals, which of course, when born, were of a tadpole form, must have immediately shed their tails and commenced respiration by lungs instead of gills. If this is so, as would appear, I regard it as one of the most remarkable phenomena we are acquainted with in nature. That from mere circumstantial contingencies an animal can escape from the conditions of its larval state, or babyhood, and put on all the characteristics of the adult, is not observed with any other animals. Now, according to all the laws of the embryonic typical structure of the tailless Batrachians, it cannot for a moment be supposed that these toads are truly of the adult form when born, but that they are true tadpoles, exactly as though to live in water; for although Nature is prophetic in her types, she is not prophetic of the contingencies of circumstances. The sudden transformation, therefore, of the tadpole to the toad would involve changes of the internal organs of the highest physiological interest,—the dispensing with the gills, and the immediate performance of the respiratory function of the lungs, organs which have to be developed in part at the same time; then there are changes in the alimentary canal, and, above all, the sudden pushing out of the legs. I am satisfied, therefore, that the subject, when fully investigated, will throw new light on the condition of the development of organs and parts, and, unless the data I have given are incorrect, is of no little importance in physiological science.

Arrests of development with animals are not uncommon; they are the causes of most of the monstrosities we observe, and I may here mention that Higginbottom, of England, has succeeded in preventing the development of the tadpole into the frog, by keeping it in repressed conditions and under relations adverse to those of its ordinary life. In this way these tadpoles may be preserved as such an indefinite length of time. But this hurrying of the developmental processes in nature is as remarkable as it has hitherto been considered impossible.*

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6. On the Signification of Cell-Segmentation. By Dr. W. I. Burnett, of Boston.

The conditions and general relations of cells, as the primordia of all organization, are now pretty well determined in physiology. They are the simple, single form of building-material out of which the fabrics or organization have their proximate and final growth. Beautiful, therefore, as well as important, is the full appreciation that this single simple body is the final expression of all that which lives, moves, and thinks, — that beauty which we worship, and intellect which we admire, is all proximately connected with the growth and use of cells. The ovum, out of which all individual life arises, is morphologically only a cell; for, as I have traced its origin in the ovaries of insects, it arises as a cell, and ever after maintains its general cell form, although it may ultimately be composed of numerous cells. Its vitellus corresponds to the cell membrane, its germinative vesicle to the nucleus, and its germinative dot to the nucleolus. The ovum, therefore, is, in a morphological point of view, only a nucleolated cell.

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* The phenomena here described are doubly interesting in comparison with those of the development of the Surinam Toad (*Pipa Americana*) recently observed by Dr. Wyman. Here the development occurs, as is well known, in sacs on the back of the female. Yet a tail is fully developed with all the swimming adaptations as in ordinary tadpoles, but it soon disappears without ever being used. Here, therefore, there is development according to a pre-established plan, without reference to the economy of life. In one case, on the other hand, there seems to be a bending of developmental processes to economy of life.

See Wyman, Silliman's Journal, XVII., May, 1854, p. 369.

Physiologically it is, of course, something higher, since it is the potential representative of the female for the production of a new being. The processes, therefore, which belong to the ovum are true cell processes, as far as all their material expressions are concerned. This point is worthy of remembrance, for it addresses that underlying harmony and unity in nature, after which the human reason is always seeking as by an intuition. The processes which the fertilized ovum undergoes just previous to the definite expression of the parts of the coming animal, are as follows.

The vitellus, together with its germinative vesicle, divides into two parts, which are likewise spheres. Each of these divided parts again divides in a like manner, and the dichotomous mode of subdivision continues until the egg has become a mulberry-like mass. Out of this mass, or rather at first on it, is formed the first traces of the new being, and the whole substance is gradually used up or consumed as the embryo is developed. This process of segmentation occurs with all ova preparatory to the formation of the embryo, whether those ova are from the highest or the lowest animals. Viewed morphologically, it appears to be the mode of working over or kneading raw materials, so that they can be converted into a special living form. The question now arises, what interpretation should be put on this peculiar phenomenon. Is it the exclusive *sequela* of impregnation, or is it only an indication of vitality seeking its individual expression? In cell studies, more or less extended, one will meet with this same phenomenon in simple isolated cells. Thus I have not unfrequently observed epithelial cells multiply in this way by dichotomous division, and in cells of cancer and other diseased growths, I have seen it most clearly and beautifully marked. It is evident, therefore, that this segmentation is not the prerogative of the ovum, but that it is pre-eminently a cell process. As for its relations to the impregnated ovum, I think they are far from being direct and sequent, for I have observed the ovum begin to segment before impregnation had taken place. But with both of these last-mentioned cases, — individual cells and the unimpregnated ovum, — this process is always limited and abortive, attaining no definite end, and although morphologically identical, it is only prophetic; it is, if I may so express myself, a yearning after that complete individuality of independent life which is attained only through the ovum. With the fertilized ovum, on the other hand, these

processes, which have been thus feebly expressed before, now assume a definiteness of action, and progress steadily towards an individual exclusive end. Forces, which were manifested indistinctly hitherto, now have a distinct course and channel for their manifestation. The segmentation of the ovum, or of cells generally, therefore, is a vitalizing process, — a means by which organizable matter reaches a higher grade, and is made fit to be the incarnation of the idea of an individual being. That this is a correct view would seem to be indicated by the fact well known in embryology, that those parts of the animal which are of the most noble nature and function, such as the nervous system, are composed of that portion of the ovum which has experienced the most extended subdivision. It is, indeed, very pleasant to trace the simplicity and beauty of the processes by which nature evolves new forms. We have seen that the ovum and the cell are morphologically identical, and therefore there is but one form for the first manifestations of life, and its subsequent development and growth. Exactly in the same way, there is a single process only by which the early conditions of vitality are expressed. This seeking after individuality by nature, this movement by a unity of plan, is a feature in natural phenomena on which the enlightened reason cannot too often dwell, since it seems to afford the human mind, through purely casual relations, a glimpse of the Grand Intelligence.

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7. On the Fatal Effects of Chloroform. By Professor E. N. Horsford, of Cambridge, Mass.

The occasional deaths that have occurred in medical practice from the use of anæsthetic agents, have, within the last two years, attracted a large measure of attention. It was earnestly maintained by some in this country, that ether had been employed in all cases without injurious effects, and that the disastrous consequences were solely due to chloroform; while in England the two agents were held in the inverse order of esteem. Others in this country advocated the use of chloric ether, in the persuasion that it was safer; while it was generally believed by those who had most to do with these agents, that the fatal results were due to idiosyncrasies of temperament on the

part of patients, or in rare cases to want of attention and judgment on the part of physicians. It had been suggested, and some experiments seemed to indicate, that the injurious effects of some samples of chloroform were due to a volatile body accompanying the chloroform, and derived from the action of bleaching-salt upon fusel oil, — a constituent of most inferior alcohols. It was conceived that this body need be present only in very small quantity to produce the fatal effect.

In the midst of this variety of explanations of the ill effects of anæsthetic agents, there appeared in the market from time to time chloroform impossible to inhale, from the presence of free chlorine and hydrochloric acid; and another, which, though not difficult to inhale, was found, upon close examination, to yield an offensive and unusual odor, as of something putrid. The latter may be easily purified by repeated agitation with sulphuric acid, and was the subject of experiment by Gregory, to whom we are indebted for the method of its purification. The former variety has not, hitherto, so far as I am aware, been the subject of special experimental inquiry.

The following investigation was undertaken with a view to determine the nature of this variety of bad chloroform, under what circumstances it might be produced, and how made pure, and, further, to determine how far the fatal effects of the administration were due to alleged impurities, and how far to other causes which have been suggested above.

The sample of bad chloroform which was made the subject of experiment was received from Dr. John Currie, an eminent pharmaceutist of New York. It was contain in a stoppered bottle, which was not quite full. The space above the liquid, and the liquid itself, presented a yellowish-green tinge. Floating upon the surface of the chloroform was a thin layer of deep yellow color, of oleaginous consistency, which, when the vessel was agitated, separated into globules, as oil does when agitated with water. Upon opening the flask, it yielded a strong odor of chlorine and hydrochloric acid.

A quantity of this bad chloroform placed in an inverted test tube over mercury, yielded more and more gaseous products, at first of a greenish tinge, but becoming in a few days colorless, displacing and forcing downward the chloroform, until now, after the lapse of nine months, several cubic inches have been evolved from a cubic inch of the liquid.

As might have been expected, chlorine and hydrochloric acid could

be entirely withdrawn by distillation from soda-lime. A quantity so purified nine months since is now perfectly good.

Another quantity in contact with cotton fibre (candle-wick) in a few days became perfectly pure, and has so remained.

A better, and a thoroughly practical and simple method, was discovered by the late Dr. Dwight, of Moscow, N. Y.* Of this method I will speak more especially further on.

It having been remarked in a letter accompanying the parcel from Dr. Currie, that the chloroform had been purified by Gregory's process, it occurred to me that the sulphuric acid might have contained a little nitric acid, and that this might have taken part in its tendency to decay. To ascertain if any form of sulphur might be present in the yellow oil floating on the surface, it was treated with fuming nitric acid and a soluble salt of baryta, but yielded no precipitate.

To ascertain if any nitrogen had, by possibility, taken on the form of ammonia, and were still present, other portions of the oily matter were, with some of the chloroform, carefully separated, and evaporated with platinum solution to dryness. They did not yield a trace of ammonio-chloride of platinum.

Unfortunately, the quantity, too small at first for quantitative analysis, was only sufficient to enable me to observe that a portion of it crystallized under certain circumstances. As upon application to the gentleman who had so kindly furnished the article experimented with, I found that no more was to be obtained, there remained only the alternative of attempting to reproduce the bad chloroform.

The atmosphere above the liquid, as above remarked, contained chlorine and hydrochloric acid, and the chloroform was pale yellow, with a shade of green. These were the indications by which experiment was to be guided.

Upon the hypothesis that this variety of bad chloroform was due to the presence of fusel oil in the alcohol, chloroform was made with alcohol containing this oil in various proportions; the water, alco-

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* An eminent physician and surgeon, who had, for more than a quarter of a century, pursued the successful practice of his profession in Moscow, N. Y., and was lost at Norwalk, having within the previous week submitted an able paper on this subject to the American Medical Association. It is to be hoped that this paper, containing much valuable information, and the result of careful experiment, will be given to the public.

hol, and bleaching-salt being taken in the proportions given by Dumas. In all cases the resulting chloroform was good.

The fusel oil of the apothecaries, treated with water and bleaching-salt in all respects as if it were alcohol, with a view to the production of chloroform, gave a product nearly like pure fusel oil in smell, and having a specific gravity, in three several preparations, of

0.8236
0.8225
0.8224

While that of pure fusel oil is 0.8138. It gave of chlorine 1.35 per cent, when evaporated to dryness with soda-lime, the chlorine in which had been previously determined. This was doubtless due to the chloroform derived from the traces of alcohol still present in the fusel oil.

That such might have been expected as the result, will be evident when it is considered that fusel oil is but slightly soluble in water, and would, of course, from the outset, float on the surface of the mixture.

Experiments made with alcohol to which was added impure methyl alcohol (wood-spirit) gave good chloroform.

Experiments with the product of distillation resulting from the mixture of pure fusel oil, water, and bleaching-salt, upon man and inferior animals, were made under varied circumstances.

A practised physician, accustomed to the administration of chloroform, inhaled the vapor of this product for fourteen minutes, without any marked anæsthetic effect, or any other effect than slight irritation of the bronchial tubes.

Two rats, one full grown, were successively subjected to the action of this agent, poured upon cotton to facilitate evaporation, the tuft of cotton and the animal being placed on the bottom of a covered beaker glass. The air was renewed from time to time with the aid of a bellows. At the end of an hour no anæsthetic effect had been produced upon the full-grown rat, and at the end of forty minutes none on the smaller animal. They were then exposed to the action of the vapor of chloroform, and in less than two minutes were insensible.

The experiment was repeated with kittens about a week old, with like results, except that they were longer in becoming insensible.

On a subsequent day two kittens were exposed to the vapor of the above body, each under a separate bell glass, while two others, in all respects similarly situated, except that the latter breathed merely con-

fined air, for one hour, without its being apparent that the vapor had produced any deleterious effect. When taken out, all appeared quite alike, so far as activity was concerned, or disposition to seek legitimate nourishment.

Several weeks later, the experiments were repeated with the same kittens, in the use of a fresh preparation of the above body, repeatedly distilled from chloride of calcium. There was in the course of an hour an appreciable lethargic effect, which was not so marked where confined air alone was inhaled, but in no instance attained such a degree that, upon the removal of the bell glasses, the animals did not at once resume, unimpaired, the possession of all their powers.

These experiments led to the conviction that fusel oil, when treated as in the manufacture of chloroform, substituting fusel oil for alcohol, is not changed, and, of course, that the fusel oil present in alcohol, in the ordinary manufacture of chloroform, does not yield a poison which, taken with the chloroform, has produced the fatal effects.

While the fusel-oil vapor and the impure chloroform which Gregory had recognized could be inhaled without difficulty, the article received from Dr. Currie, by its violent irritation, closed the glottis almost instantly.

Upon the presumption that the bad attribute was due to the mode of purification, a quantity of good chloroform was repeatedly distilled from concentrated sulphuric acid, and another from chloride of calcium. The products in both cases were perfectly good, and are so still, now nine months from the date of distillation.

Having tried alcohol of various degrees of purity and strength, and having subjected good chloroform repeatedly to different methods of purification, it remained to try different samples of bleaching-salt with varying proportions of alcohol. Accordingly, several varieties in the market were procured, and a series of experiments undertaken by Mr. Gould, of the Laboratory of the Lawrence Scientific School.

The combinations of alcohol, bleaching-salt, water, temperature, time and mode of distillation, made to meet the inquiry, required above fifty successive preparations of chloroform.

From these and the foregoing experiments it appears, —

1st. That good chloroform does not spontaneously change in a period of nine months.

2d. That the bad chloroform, containing free chlorine and hydro-

chloric acid, may be produced by using bleaching-salt of great strength, with a quantity of alcohol disproportionately small.

3d. That the bad chloroform may be produced by receiving the distillate into water, so as immediately to withdraw the alcohol from the chloroform.

4th. That the bad chloroform may be produced by passing chlorine directly into chloroform.

5th. That no formula for its manufacture can be relied upon as a guide, since bleaching-salts vary in strength when derived from different factories, and vary with age. In the foregoing experiments the range is from 15 to 30 per cent of available chlorine.

6th. That quicklime added to the mixture does not promote the economy of manufacture.

7th. That the chlorine and hydrochloric acid of bad chloroform, as observed by Dr. Dwight, may be removed by agitation with a little alcohol.

8th. That the ill effects observed in the administration of chloroform are not due to the presence of chlorine, as the irritation is such, when it is attempted to inhale it, as to prevent inhalation altogether.

9th. That the ill effects are not due to any poisonous product arising from the action of bleaching-salt on the small quantity of fusel oil in the alcohol employed in the manufacture of chloroform.

10th. That the ill effects are due to the peculiarities of constitution or temperament of some patients, and in a few rare cases to want of attention or judgment on the part of the person administering it.

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8. On the Histology of Red Blood. By Professor J. L. Riddell, of New Orleans.

Having examined with much care through Spencer's best sixteenth, of *one hundred and seventy-four degrees*' angle of aperture, many samples of blood from different animals, with and without the influence of chemical agents added, I have come to certain conclusions respecting the histology of the red blood of the Vertebrata, which I desire to lay before microscopists and physiologists for their consideration.

I have concluded to reverse the common custom, and present first the conclusions, and then a selection of observations, illustrated by figures drawn as magnified *one thousand diameters*, upon which these conclusions are founded. It is proper to state, that some account of this matter appeared last year in the May number of the New Orleans Medical and Surgical Journal.

Summary.

1st. All red blood-corpuscles, whose function it is to absorb oxygen for the use of the animal system, whatever their diversities of size, shape, and appearance in different animals, possess a similar, or at least an analogous structure.

2d. They all have a *nucleus*, or its analogue, or internal composite cell or cells, similar in appearance to other animal cells, and similar to algoid cells; consisting of associated smaller cells or *nucleoli*, held in an appropriate common envelope, and these smaller cells or nucleoli containing again others still smaller.

3d. Around the whole nucleus is wrapped the distinctive tissue of the red corpuscle, which I call the *pallium vesiculatum*. In this is found the red coloring-matter. The function of this tissue is to absorb oxygen in respiration for the use of the system. In its structure are multitudes of exceedingly minute spherical vesicles of nearly uniform size for each species of animal. In the blood of man, etc., this pallium fits closely upon the nucleus; while in the blood of the Amphiuma, etc., the pallium forms a broad margin around the nucleus.

Selected Observations.

1. Blood of the Alligator, procured by Dr. Hale at the vivisection of an alligator, May 12, 1852, by Dr. B. Dowler.

Fig. 1.

Fig. 2.

Fig. 1. Natural appearance of the red corpuscles, floating in the serum; no internal structure being apparent.

Fig. 2. Red corpuscles seen edgewise.

Fig. 3.

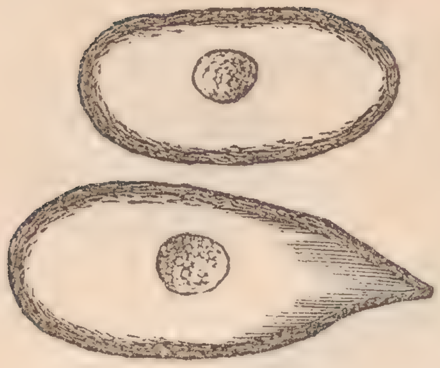

Fig. 4.

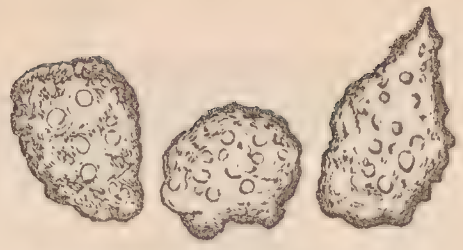

Fig. 5.

Fig. 3. Nuclei made apparent in the red corpuscles, by adding to the serum a solution of carbonate of soda. The pointed outline is not due to the reagent added; it is frequently witnessed in the normal blood.

Fig. 4. White corpuscles floating in the serum; apt to be seen in groups, as they seem to manifest a segregative tendency.

Fig. 5. By bringing in contact with the blood a watery solution of bromine, weak tincture of iodine, or a solution of chloride of platinum, the membranous expanse of the red corpuscles exhibits a granulated appearance, as here shown. (*Pallium vesiculatum.*)

Fig. 6.

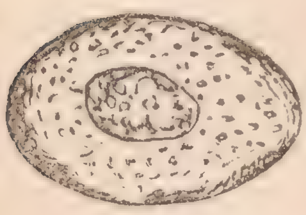

Fig. 6. Red blood-corpuscle of the Tree-frog, treated with salt and vinegar so as to make apparent the *pallium vesiculatum*, which here, from the minuteness of the vesicles, has merely a well-marked granular aspect.

Memorandum. It is not probable that larger blood-corpuscles have ever been examined than these of the Amphiuma, they frequently exceeding in length the two hundred and fiftieth part of an inch. The nearest approach on record, so far as I am aware, is the blood of the Siren, a batrachian reptile closely allied to the Amphiuma, in which the corpuscles are reported by Gulliver to be a four hundred and twentieth of an inch long. Owen (Lectures Comp. Anat., II. 13) represents the corpuscles of the Siren blood to be one three-hundredth of an inch long. The Amphiuma blood-corpuscles are not only longer, but, judging from the figures in Owen, and in Hassall's Micros. Anat., much broader in proportion, and therefore considerably larger. Their average size, cubically measured, is more than 1,200 times as great as the corpuscles of human blood.

2. Blood of the *Amphiuma tridactylum*, Cuv., a batrachian reptile, common in the muddy swamps of the Louisiana Delta, often seen

four or five feet long, and almost black in color. The negroes about New Orleans call it the Congo Snake.

Fig. 7.

Fig. 7. Natural appearance of red corpuscle, seen upon its broad surface; the nucleus apparent.

Fig. 8.

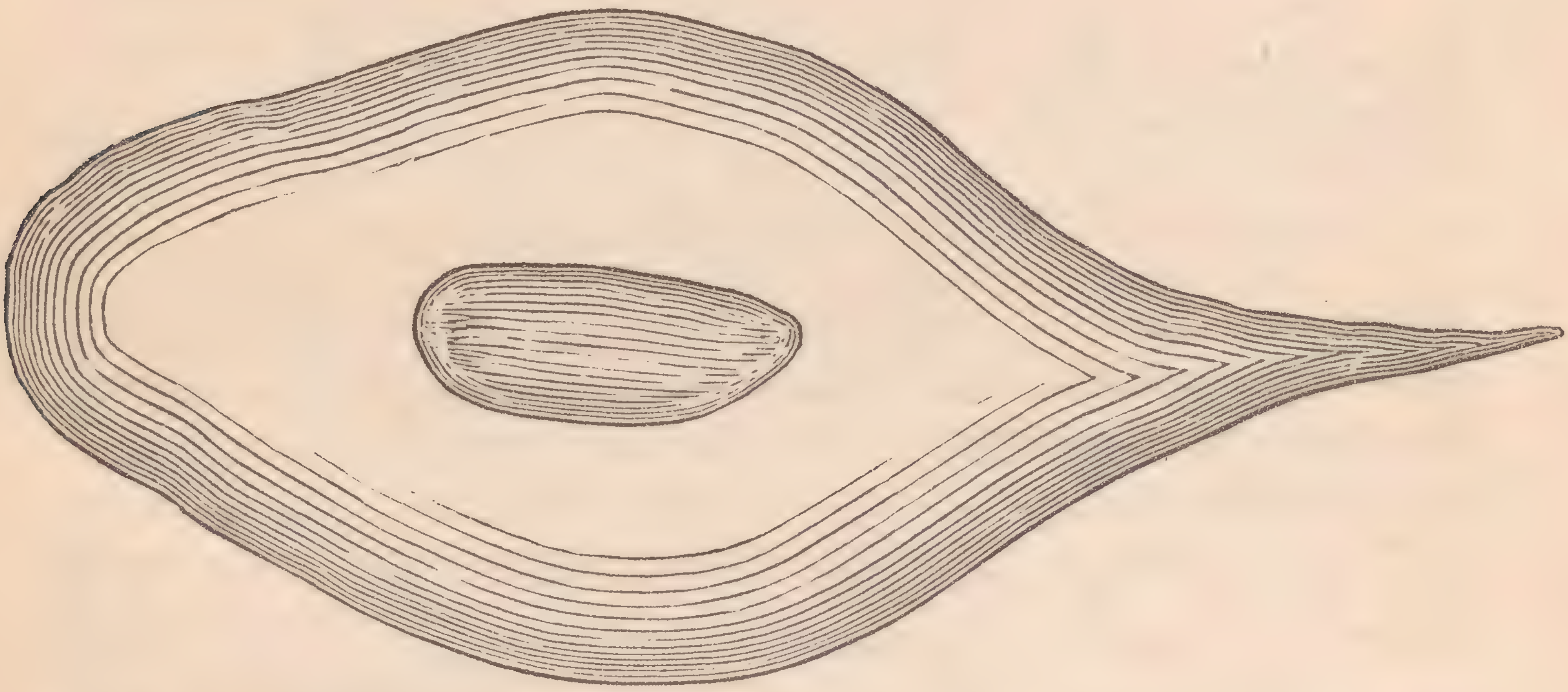

Fig. 8. Red corpuscle with a pointed extremity. These, and others with both extremities pointed, are not unfrequent. More rarely, these attenuated ends are symmetrically bulged out and rounded off.

Fig. 9.

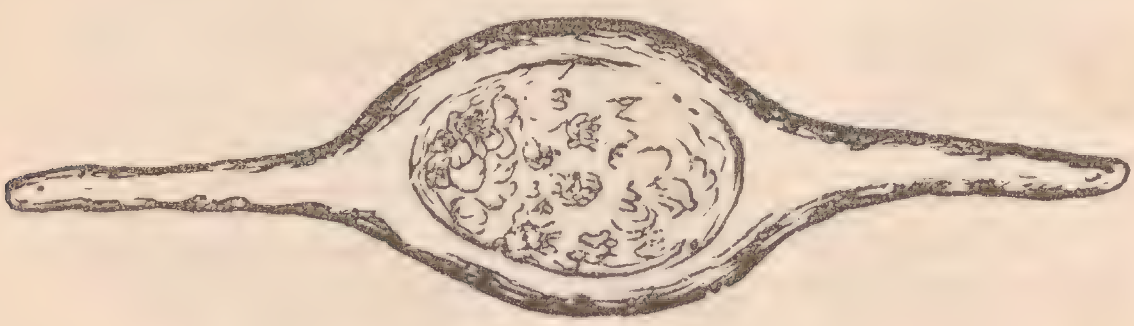

Fig. 9. The red corpuscles seen edgewise.

Fig. 10.

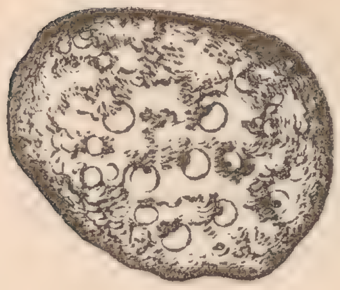

Fig. 10. White corpuscles of the blood of Amphiuma.

Fig. 11.

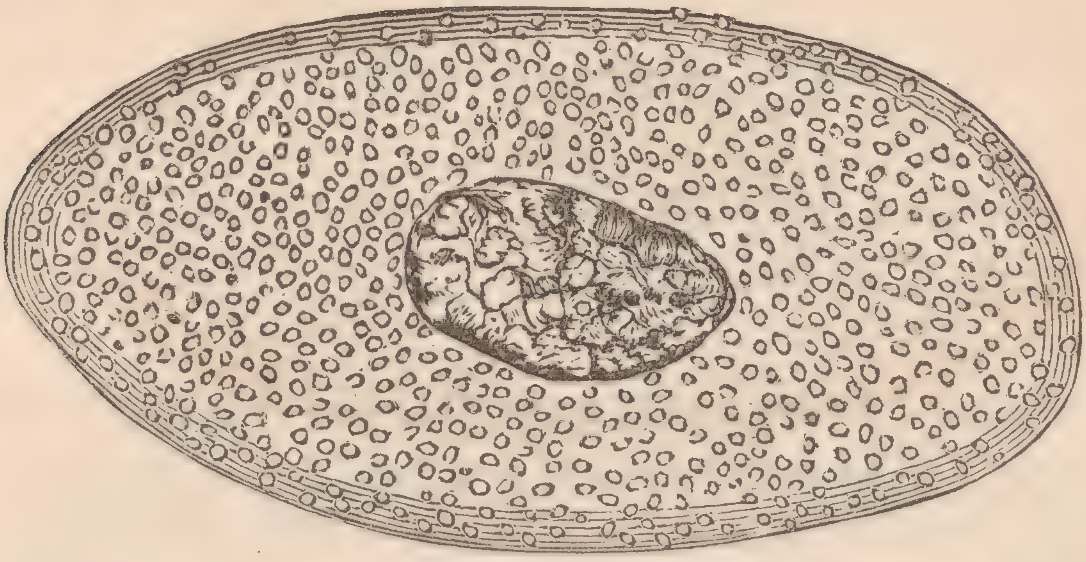

Fig. 11. Appearance of the red corpuscle after adding to the blood a solution of common salt, and subsequently adding acetic acid, according to the valuable plan of Professor James Jones. Other methods of chemical treatment bring out similar appearances, but not always with the same distinctness. We here have,— 1st, a composite cell, strictly analogous to other animal and algoid cells, namely, the *nucleus,* containing numerous nucleoli, which in turn are visibly filled with nucleolar and granular contents; 2d, the *vesiculate mantle, pallium vesiculatum,* or specific peculiarity of a red corpuscle, containing in its texture a great multitude of minute spherical vesicles of similar size; being the site of the red color of the blood, and the substance which probably serves as an oxygen-bearer from the lungs to the capillary tissues.

Fig. 12.

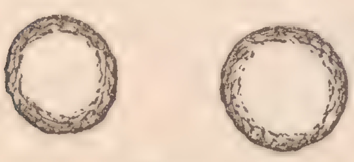

Fig. 12. Red corpuscles of human blood, as seen when just drawn; lenticular, apparently structureless discs.

Fig. 13.

Fig. 13. The same, with the margin slightly crenulated, an appearauce due to a slight contraction of the *pallium* upon the nucleolar contents. This appearance ensues spontaneously, and is probably due to exosmosis.

Fig. 14.

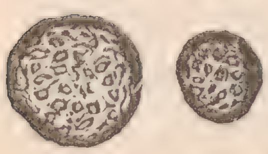

Fig. 14. White corpuscles of human blood.

Fig. 15.

Fig. 15. The stellated or mulberry form of the red corpuscles of human blood, occurring spontaneously when the serum has been somewhat concentrated by evaporation. Here the *pallium* has so contracted, as to adapt itself to every protuberance and sinuosity of the nucleus, making the several *nucleoli* (a dozen or so) very apparent.

Fig. 16.

Fig. 16. By treating human blood with weak tincture of iodine, or with bromine, or with salt and afterwards vinegar, the *pallium vesiculatum*, as here shown, is made apparent, presenting fine uniform granulations. Under this treatment the nucleoli, not here represented, can also be distinctly seen without the contraction of the pallium, as in Fig. 15.

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9. On the Origin of Capillary Bloodvessels. By Professor J. L. Riddell, of New Orleans.

From repeated observations upon the development of the ova of the Tree-frog (*Hyla arborea*, Lau.), it has appeared obvious to me that the earliest embryonic blood-corpuscles differ from the structure cells, in being unattached and free to be moved, rather than in size, color, or histographic peculiarity. It is also obvious that the smaller bloodvessels (I cannot speak from observation regarding the larger ones) are of secondary formation, in regard to their first burden of blood being produced interstitially among or between the structure cells, in all probability by a plastic deposit from the liquor sanguinis itself; just as the origin of the bed and banks of a sediment-bearing river, meandering through an alluvial plain, is unmistakably to be referred to the river itself. This genesis of bloodvessels, demonstrable in the progressive development of frog spawn into tadpoles, may, by analogy, be inferred to occur in other departments of the animal kingdom.

I will now proceed to lay before you selections from a single set of observations, commenced on the 3d of July, 1852, with a mass of ova voided that day. The illustrative figures are drawn as magnified *one thousand diameters*.

These figures, with brief explanations, appeared in the September number of the New Orleans Medical and Surgical Journal, 1852.

The freshly voided ovum contained a black spherule one twentieth of an inch in diameter, surrounded by a transparent, gelatinous envelope one fifth of an inch in diameter. Now, it matters little in what order the following figures are explained; for despite the absence of regular order, the evidence on which the foregoing inferences rest will, I trust, be sufficiently apparent.

Fig. 1.

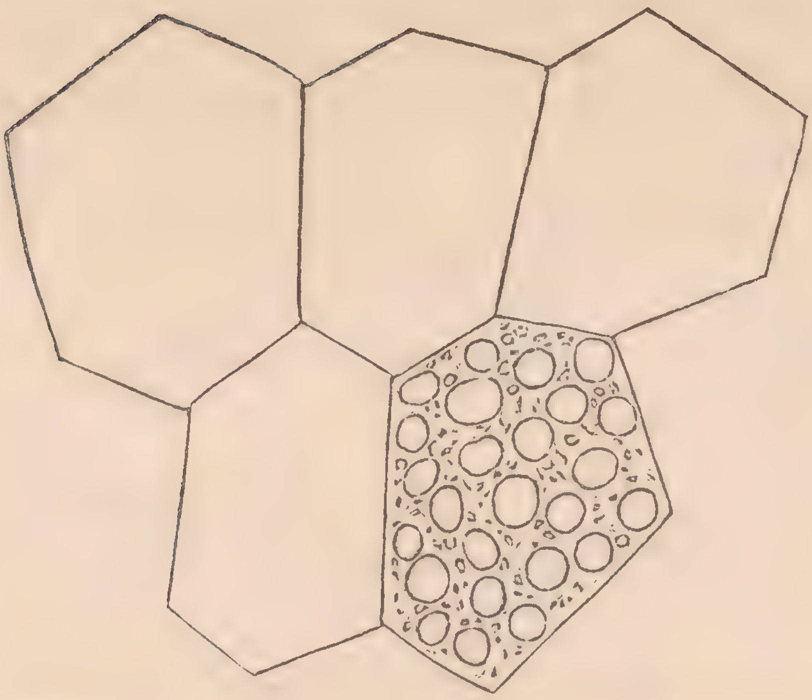

Fig. 1. Superficial arrangement of the cells in the tail and skin of the body, [seen July 4,] no interstitial spaces being apparent. Material for assimilation is evidently supplied to these cells by simple endosmosis or imbibition.

Fig. 2.

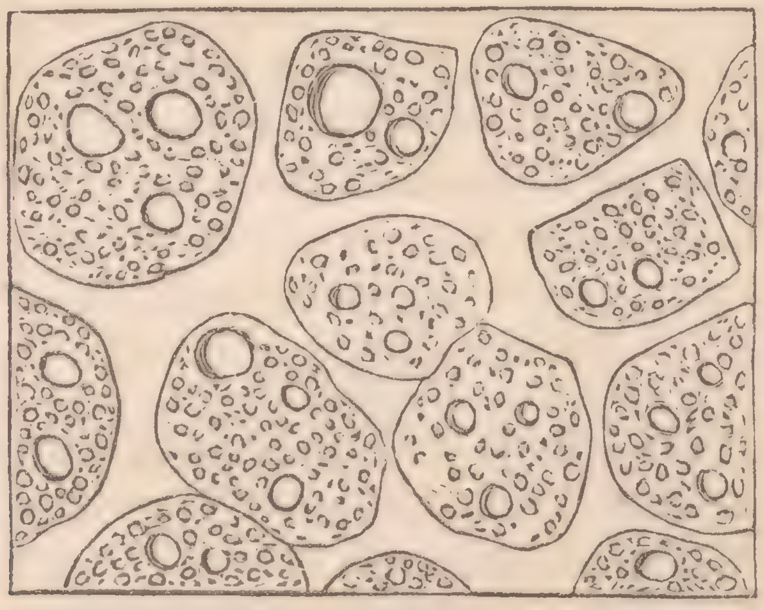

Fig. 2. Cellular structure seen in the tail of a fourth-day tadpole [July 6], showing the superficial cells more or less removed from each other, leaving intercellular spaces. The adjacent deeper layers of cells being out of the focus, and not here represented, are possessed of a similar mutual arrangement, there being interstitial spaces. This is evidently the effect, in part, of growth, and in part of the intromission of a clear fluid between the adjacent cell walls. Two days earlier, no intercellular spaces were seen. Vide Fig. 1.

Fig. 3.

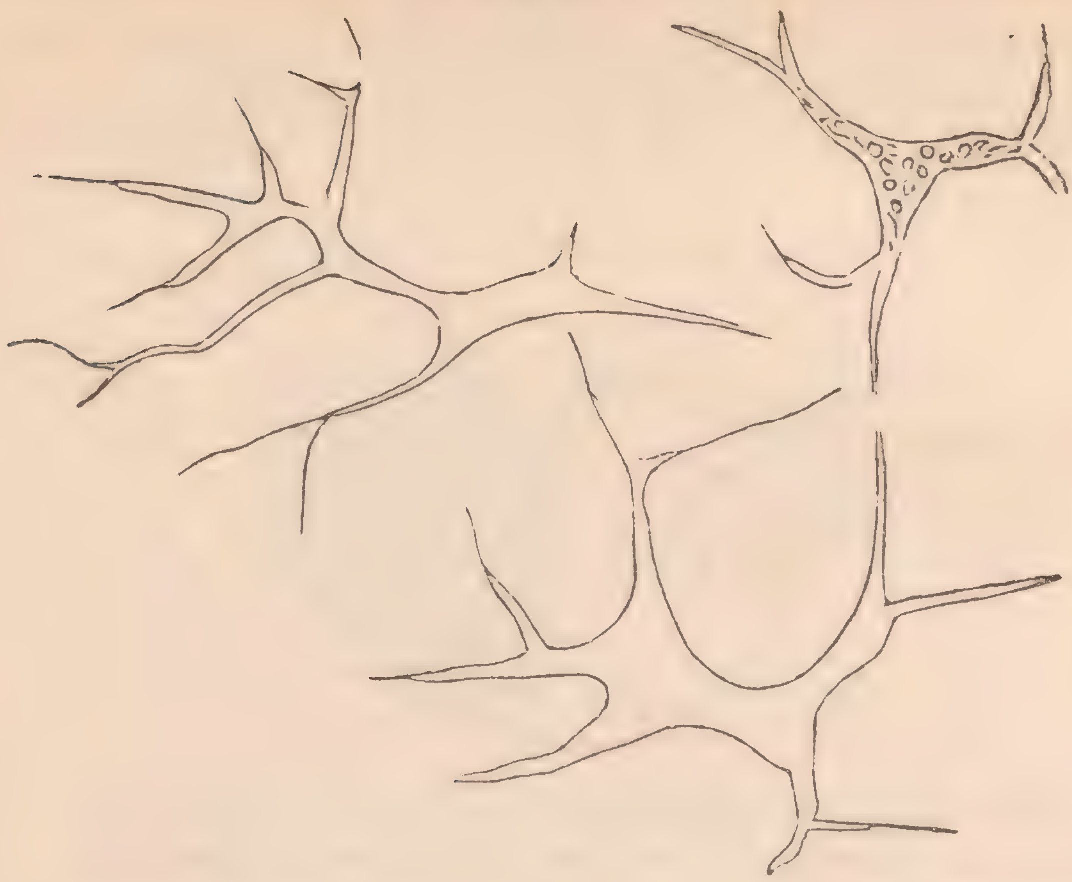

Fig. 3. Incipient capillary vessels. The same tissue as the foregoing (Fig. 2), examined three days later, presents the intercellular spaces modified, as represented in these figures. It would seem that the tension or internal pressure of the fluids of the growing tadpole forces a clear liquid between the cells; that there is thus a commencement of intercellular lymphatic circulation. This fluid probably leaves a trace of plastic deposit in its path, and thus produces the curiously ramified structures here represented. There are, in effect, vessels branching in all directions, and communicating variously with each other, (and doubtless also with the capillaries carrying corpuscles subsequently,) by channels often less than one 50,000th of an inch in thickness. In the enlarged portions, fine granular matter is sometimes seen. But no contained bodies, like proper cell nucleoli, or Schwann and Schleiden's cytoblasts, are at any time discoverable in these vessels, while they continue to carry clear fluid.

Schwann, in his original and very able memoir of researches into the origin of animal cells, is inclined to regard these structures as true cells. He describes them as stellate cells, alleging that he occasionally saw what he supposed to be the nuclei from which they were generated. Further on I will advert to what I suppose misled him. Carpenter, and other writers on physiology, have adopted Schwann's suggestion in their attempt to account for the origin of the capillary vessels.

As the development and growth of the tissues proceed, the incipient vessels, containing and transporting a transparent fluid, seem to be distended by the tension of their contents, and by the impetus of the blood-corpuscles and serum. This impetus is obviously derived from the action of the heart, and from the general muscular movements. The corpuscles are forced into the mouths of the incipient vessels. Some of them form valves, as in Fig. 4, *e*. The corpuscles, unable to return by their path of entrance, are forced to open for themselves, through the ramifications of the incipient vessels, a new channel of circulation.

It is extremely probable, that the first single file of corpuscles, travelling, as well as subsequent ones, in the newly distended vessel, deposits upon the wall of the vessel a portion of plastic material, which tends to give body and limited thickness or diameter to it. Hence the probable origin of the capillary vessels.

Fig. 4.

Fig. 4. Capillary vessels for the circulation of blood-corpuscles.

a. Red corpuscles, appearing, as seen through the microscope, of a pale brownish-yellow, moving through the capillary vessels, travelling really at the rate of an inch in from five to fifteen minutes.

b. White blood-corpuscle.

e. Red corpuscles which have become attached to the wall of the vessel. I have often seen them run aground in this manner. Sometimes they get loose and resume their voyage; acquiring by the accident a more or less attenuated caudal appendage. Such must be the origin of the caudated corpuscles so frequently seen in the blood of the Reptilia, and sometimes in the blood of man. In other instances, they seem to become permanently attached, forming, in effect, a sort of valve, which is obviously adequate to act in the way of preventing regurgitation in the smaller capillaries.

f. Blood-corpuscle attached along its whole length to the wall of the vessel, probably mistaken by Schwann for a cytoblast.

c, d. Blood-corpuscles as seen bending around angles. The red corpuscles are remarkably flexible and plastic.

As directly and intimately connected with this subject, I have thought it would be appropriate to conclude this paper with some

additional remarks on the *motive power causing the circulation of the blood-corpuscles.*

In reference to the cause or causes of this capillary circulation of corpuscles in the living tadpole, I entertain no doubt whatever in referring the phenomena entirely to muscular action. Because, —

1st. The action of the heart can be clearly seen by a half-inch lens, and its pulsations timed.

2d. With different powers of the microscope, the blood can be visibly traced even to the extreme capillaries, manifesting, in a decreasing degree, the impulse of the heart in quickened movement the whole way.

3d. When a stasis or obstruction occurs in the extreme capillaries, during the life of the tadpole, the heart's impulse becomes palpably apparent, in a slight advance and retreat of the corpuscles, synchronous with the contractions of that organ.

4th. General muscular action, by successively compressing and distending different parts, must tend to give motion to the contained fluids; and, as valves occur, not only in the veins, but even in the capillaries* (Fig. 4, *e*), blood when made to move is of necessity mostly moved in its normal direction, from the arteries, through the capillaries, into the veins.

5th. Considering the normal velocity of blood moving in the capillaries an inch in eight or ten minutes, we should expect comparatively a very small obstruction from the friction of the moving fluid. Other things being equal, the resistance would be nearly in a duplicate ratio to the velocity.

6th. Any degree of vital, endosmotic, capillary, or chemical force, supposed to be exerted between the fluid or the corpuscles on the one hand, and the walls of the containing capillary vessels, or the subjacent tissues, on the other, could never be expected to cause the blood to move one way rather than the other; for it would be a force necessarily acting at right angles to the path of movement.

7th. The extreme capillaries seem to be mechanically passive, suffering in rare cases a distention synchronous with the pulsations

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* In some of the capillaries, no valves were observable. In these I have often seen the current of blood change its direction, especially while the tadpole was dying.

of the heart, to an extent barely appreciable; usually giving transit to the blood, without observable change of form or dimensions.

I am aware that opinions very different from what I have here expressed, respecting the agencies here employed in circulating blood, have been lately advocated by writers of great ability and high standing. Their opinions seem to have been confused, by comprising in the same point of view two phenomena in every respect wholly distinct; namely, the continuous circulation of organized corpuscles [blood], which corpuscles may be seen through tubes or vessels of measurable caliber, — a purely vital phenomenon, only witnessed in animal beings that are at least large enough to be visible to the naked eye; and a purely physical movement (not a continuous circulation) of fluids, in which no visible corpuscles are necessarily concerned. Of this nature are imbibition, endosmosis, and exosmosis, in animals and plants, and even in unorganized matter. Endosmotic imbibition at the rootlets, and exosmotic evaporation from the leaves, contribute thus, efficiently, to the ascent of sap in trees. And wherever organized cells, in a living condition, are in the active performance of their accustomed functions, these physical agencies are seen to be subservient to the processes of vitality, in supplying material and disposing of products.

In most of the processes of animal life, the oxidation of carbon takes place, requiring the constant supply in some form of oxygen. The microscopic animalcule can absorb directly enough of this agent from surrounding air or water, and for this purpose needs and possesses no corpuscular circulation. The aquatic worm, barely visible to the naked eye, will generally present, under the microscope, an alternate or reciprocal flow of corpuscles, from near one extremity of his body to the other, then back again in the same simple channel. It is only in animal beings of a larger size, where cell life is in progress at such profound depths from the surface as to preclude the access of air by direct imbibition, that a complicated and special circulatory apparatus becomes necessary.

10. On some Points in the History of Gordius. By S. N. Sanford, of Granville, Ohio.

About the middle of June (1853) a neighbor stepped upon and crushed a cricket, from which was immediately seen to crawl a *Gordius*, between ten and twelve inches in length. This entozoön being of a species unknown to my friend, and its lodging-place within a *cricket* seeming to him a novel one, the specimen was placed in water, and after three days sent to me, with a statement of the circumstances of its discovery.

After keeping it several days, during which time it evidently became daily less and less active, I sealed it in a vial of alcohol, and sent it to Professor Baird, of the Smithsonian Institution. He kindly and promptly replied to my inquiries, and added that this fact, though previously known, was of interest, as tending to settle the question of the successive changes of the abode of the Gordius.

A few days later, a sister of mine, seeing a *dead* cricket upon the floor, threw it into a teacup; and in a few minutes after, chancing to look into the cup, found, to her surprise, another specimen of the same kind coiled around the cricket on the inner side of the cup. This was set aside for me, and I found it *smaller* and less *sprightly* than the former.

A few days since, the neighbor above referred to,* seeing a cricket upon the floor, which appeared stupid, purposely stepped upon it, and then examined its crushed body. He found another specimen of the same kind, between six and eight inches in length, and, what is far more interesting, some *ten or twelve* small Gordii, nearly equal in size, and not more than *one eighth of an inch* in length.

Hoping that this simple statement of facts may not prove entirely devoid of interest to the members of the Association, I have, at the suggestion of Professor Baird, to-day hastily prepared it. Without presuming to express an opinion on the question of the origin of the Gordius, I will simply say, that the third and last of the above observations *seems* to have an *unmistakable* bearing upon that question, and in favor of its internal origin.

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* The gentleman referred to is Mr. Charles Woods, of Granville, Ohio.

C. PHILOLOGY.

1. Investigation of the Power of the Greek Z, by means of Phonetic Laws. By Professor S. S. Haldeman, of Columbia, Pa.

Those who attempt to investigate the powers of the Greek and Latin alphabetic characters with the aid of the ancient grammarians, are met by a difficulty, in some places, which at first seems insurmountable; namely, the different readings of the text, which will allow the same passage to be quoted to maintain the most opposite views. This may be attributed to the fact, that, in the course of the gradual changes to which language is subject, some of the letters would acquire new powers, or be used in a perverted sense; and so far as these powers and characters became naturally associated in the mind, would the descriptions of the ancients seem obscure when treating of a character thus perverted.

Let there be, for example, a collocation of the English consonants *zd* (as in wi*sd*om, E*sd*ras) in Greek and Latin, and let this double sound be represented by the single character ζ, writing E*z*ras, but pronouncing E*zd*ras. Let the same character ζ be gradually associated with the power of English *dz*, as in Italian, and to such an extent that Greek and Latin words will be naturally read like Italian ones. The later Italian scholar, in learning from the ancients that the Greek ζ was a double letter composed of *s* and *d*, would be likely to corrupt the text, under the idea of correcting it, to make the *s* follow the *d*; and this is just the condition in which we find the history of Greek Zeta.

Important characteristics often lie in the combinations of the vocal elements, and as we cannot have a philosophical view of a language unless its exact condition be known, we must endeavor to solve a question of this kind with the collateral aid of the mechanical structure of the language, — depending upon the physical action of the vocal organs, — an aid which grammarians are not inclined to make use of.

Some moderns, including the Greeks, doubt the fact of ζ having

been a double letter, although the ancient grammarians are explicit upon this point. These Greeks contend that the comparison of ζ to *s* and *d* merely meant English *z*; as if, to give a native of Somersetshire (who pronounces *s* as English *z*) an idea of the hissing sound of *s*, we were to tell him that it is composed of his perversion (English *z*) and *t*.

This argument, which deserves attention, has not been placed in its strongest light by its advocates. As *d* and *b* are aspirated in Modern Greek, *d* and *b* cannot be represented except by a new letter, or an expedient, as $\nu\delta$ and $\mu\beta$; so, if ζ had its English sound, and had to be explained by other letters, *s* and *d* would answer the purpose very well, because, English *z* being a vocal *s*, the character of the latter would answer if a vocal letter like *d* were added to indicate this vocal quality.

Mr. E. A. Sophocles argues that ζ was not a double letter, because the Ionians used it, although they separated the other double letters, writing $\chi\sigma$ for ξ, and $\phi\sigma$ for ψ. But as ξ is normally $\kappa\sigma$ and not $\chi\sigma$, it would have been erroneous to use it for the latter dialectic variation. Let us illustrate by a modern example. In dialectic variations the German word *nichts* loses the *t*, making *nichs*, which would be equivalent to a Bœotian form, as if $\nu\iota\chi s$; or, simulating a Doric form, it would be *niks*, as if $\nu\iota\kappa s$; and it is evident that, whilst the use of the character ξ would be of doubtful propriety in the former, it would be proper in the latter.

But the best reason for not writing the elements of ζ separately, even when $\kappa\sigma$ and $\pi\sigma$ were thus represented, was the impossibility of doing so, from the fact that it was not composed of any two letters of the alphabet, except in the Doric and Æolic dialects, where it was separated into σ and δ, (not δ and σ), unless when initial, where ζ probably had its normal power of English *zd*, and consequently was not strictly represented by *sd*.

In the Doric combination $\sigma\delta$, therefore, the σ may be presumed to have had its pure or hissing sound. But the initial element of normal ζ was not pure σ, but English and French *z*, for which there was *no separate character* in Greek and Latin, as there is not in Italian and Spanish. In Modern Greek, the δ part of ζ having been lost, the character remains with its English power, which is some evidence in favor of this power being part of its original sound.

This explains another difficulty upon which the Modern Greeks insist; namely, that if ζ had been *sd*, the Romans could have thus represented it, but we are told that the sound was unknown to Latin, that is, the sound of the part represented by English *z*. Nevertheless, in giving some idea of its double power, the ancient grammarians made the nearest approximation afforded by their alphabet.

Were ζ composed of Latin *ds*, it would be likely to be common as a final in Greek, where σ is the commonest final consonant, occurring detached, and in ξ and ψ. Surd consonants are more readily produced than sonant ones, and in languages where both phases are present, a surd combination may be expected with its corresponding sonant; consequently, if ζ were δσ, we should be able to find τσ, which, however, does not occur.

We will now reverse the case, and see what part the ζ will play in Greek, if, as English *zd*, we assume that it ends with δ. In certain inflections, when a δ or τ would fall before σ, tending to form the un-Greek combination *ds* or *ts*, the δ or τ was dropped, as in ἔπεσον, not ἔπετσον; and φροντίσω, not φροντίζσω, where δ, as the last element of ζ, would be brought before σ. In ᾖκα for ᾖδκα an elision of δ before κ is shown, and the same thing takes place with the final δ of ζ in πεφρόντικα for πεφρόντιζκα. If δ before μ has a tendency to become σ, it will be likely to do so when it forms part of ζ; hence we find ἧσμαι for ἧδμαι paralleled by φρόντισμα for φρόντιζμα. In this case, if ζ were *ds*, there would be no need of change, as that would form the euphonious Greek combination σμ.

If δ is elided before σ, another dental, ν (itself a nasal *d*), may be expected to exhibit the same law. We need not be surprised, therefore, to find σύζυγος for σύνζυγος, a change which aids in confirming the view that ζ was not *ds*, as in that case ν would have been brought before δ, forming an euphonious Greek combination requiring no change.

As a Greek or Roman could write no foreign word containing a sound unknown to him, he represented such sounds by the nearest approach his alphabet afforded, writing *Arphaxad*, although the original contained *k-sh*. This adaptation accounts for the Greek form Ἀζῶτιαι being used for two Hebrew originals, in one of which ζ is the representative of the sounds *sh-d*, and in the other of *s-d*, and we find *Esdras* written both Ἔζρα and Ἔσδρας.

As the Greeks and Romans looked upon other nations as barbarians, and their languages as barbarous, they did not care to pronounce a sound they must have had but few opportunities to hear; and being unable to pronounce it without practice, the Hebrew *shin* naturally became S. The moderns, however, who both write Latin and pronounce English *sh*, have a difficulty in representing Latinized geographical and personal names; and wishing to preserve the sound in question, they do not like to convert it into S, but fall into the greater error of fancying that the sound may be represented, and in Latin, by English *sh*, French *ch*, or German *sch*, which, however, can have no such power in Latin, where *sh* must have the same power as in mis*h*ap, *ch* that of Greek χ, and *sch* that of Greek $\sigma\chi$, or σ followed by χ, — a result perfectly barbarous. In giving several geographical names to insects, I have used the old long S of European typography, which had already been used in printing Latin.

D. PRACTICAL SCIENCE.

MECHANICS.

1. On the Resistance of the Vertical Plates of Tubular Bridges. By Herman Haupt, of Philadelphia.

The great desideratum in the construction of bridges is an arrangement of parts that, with a given weight of material, will possess the most efficient power of resistance. An arch fulfils the condition of maximum resistance with a given weight of material when the distribution of the load is constant. With a permanent weight, whether uniformly distributed or not, there is always a curve of equilibrium, and a given amount or weight of material arranged to conform to this curve will give the maximum of resistance which the material is capable of opposing.

But for ordinary bridges, especially for railroad purposes, there can be no curve of equilibrium; the load is not only variable, but

very great in proportion to the weight of the structure ; and were an equilibrium possible in one position, it could remain stable only for an instant, as the transit of the load to another position would at once disturb it.

It is evident, therefore, that the principle of equilibration is applicable only to aqueducts or other structures upon which the loads are nearly constant, and that, to render an arch capable of sustaining a variable load, it must either be made so deep that the variations of the curve of pressure will never pass outside the arch, or it must be combined with a resisting truss capable of effectually opposing any tendency to change of figure.

Most of the bridges in general use are illustrations of this species of combination ; but the main reliance is usually placed upon the truss, the arch being used merely as an auxiliary.

When wood is used, this increase of material is not objectionable, except so far as it increases unnecessarily the weight of the structure ; but when iron is employed, it becomes essentially important that the distribution of the forces should be understood, the strains upon all parts of the structure accurately determined, and the dimensions correctly proportioned to the required resistances.

The principles involved in these calculations have already been given to the public, in a work on the general Theory of Bridge Construction, and no further reference to them is required, except to state that an application of these principles would lead to a radical change in the manner of constructing bridge trusses, and, instead of using an arch as an auxiliary to a truss, the arch would be made the chief dependence, and the truss employed to resist the action of variable loads, and prevent change of figure in the arch during their passage over the bridge. When disposed in this manner, a comparatively small amount of material will oppose an efficient resistance either to vibration, flexure, or fracture.

In tubular iron bridges, which have recently attracted much attention, no difficulty is found in determining the strain upon the horizontal tables, and in proportioning them correctly ; but the dimensions of the vertical ribs and the best manner of stiffening them will depend not only upon the magnitude of the weight, but upon the mode of application.

If the weight supposed be applied at the middle of a tubular bridge,

its action would tend to shorten the diagonal extending from the middle of the top chord or table to the end of the bottom table, and at the same time to elongate the opposite diagonal from the middle of the bottom table to the end of the top table; as the strain upon the last diagonal would be tensile, its ability to resist would be nearly as the strength of the material; but the opposite diagonal, being subjected to compressive forces, would, if it formed a line of a thin plate of metal, be unable to oppose any efficient resistance, and the bridge would probably fail in the direction of this diagonal. It becomes necessary, therefore, to provide means for stiffening the ribs, and this is usually effected by riveting to them, vertically, pieces of T iron at short intervals. By these additions the distribution of the strains is changed, and the action of the parts is similar to that of a truss on the Pratt plan; the vertical stiffeners taking the place of the posts, and the lines of sheet-iron connecting the opposite angles of any panel having a corresponding action to the ordinary diagonal rods.

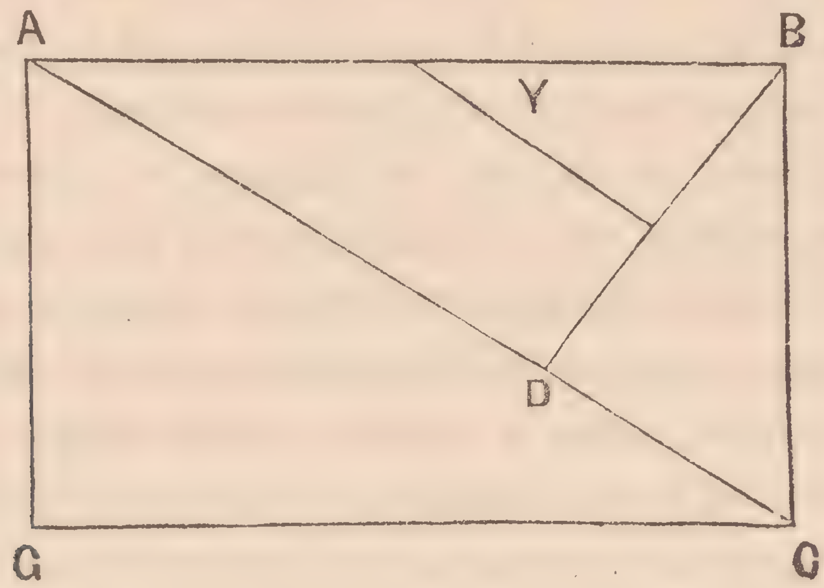
The resistance which such an arrangement is capable of opposing, it is proposed to determine from the following considerations.

In the general theory of bridge construction, it has been demonstrated that, where the load is uniformly distributed, the vertical strain increases uniformly from the middle to the ends. The strain upon the diagonal of any panel will be as the length of the diagonal to the length of the vertical side, and also as its distance from the middle of the truss, the maximum vertical strain being of course equal to half the weight of the bridge and its load, increased by the momentum due to motion and vibration when the structure is not rigid.

It has also been shown that the vertical and diagonal strains upon any panel, which are always proportioned to each other in a fixed ratio, are also in proportion to the degree of angular motion or change of figure in any panel caused by flexure; and, as the settling of a bridge to the figure of an inverted arc would not change the rectangular shape of the middle panel, the vertical and diagonal strains upon this panel would be theoretically nothing, while at the end of the truss the variation from a rectangle, and consequently the intensity of the vertical and diagonal strains, would be greatest.

Knowing the whole weight of the structure, the strain upon any panel is readily determined; and it is now proposed to inquire what will be the resistance of a sheet of metal securely attached to all the

sides of a rectangle in comparison with an equal quantity of material arranged in the form of diagonal rods. It is evident that the consideration of this question will afford the means of comparing the relative efficiency of the distribution of the material in the side plates of tubular bridges, as compared with diagonal panel rods.



Let $A\ C$, in the annexed figure, represent the diagonal, which is subjected to a tensile strain, and R the resistance per unit of area which the material is capable of opposing, and (a) its perpendicular distance $B\ D$ from the fulcrum B. The strain upon a unit at the distance x will be in proportion to this distance, and will be represented by $\frac{R}{a}x$. But as the efficiency of the resistance will be in proportion to the leverage, which is x, the whole effect of a unit of material at the distance x will be expressed by $\frac{R}{a}x^2$, and the sum of the resistances along the line $B\ D$, obtained by the usual method of integration, will be $\frac{1}{3}\ R\,a^2$, or $\frac{1}{3}\ R\,a \times a$. As R represents the resistance of the material per unit of area, and (a) the number of units in the distance $B\ D$, $R\,a$ would represent the resistance of a rod whose area is a, and $R\,a \times a$, its moment of resistance at the distance $B\ D$. As the resistance of the triangle $A\ G\ C$ is necessarily equal to that of $A\ B\ C$, the whole resistance of the rectangle $A\ B\ C\ G$ will be $\frac{2}{3}\ R\,a^2$. The resistance expressed by $\frac{2}{3}\ R\,a^2$ is obtained by the use of an area of material represented by $A\ C \times B\ D$. If this same amount of material should be placed in the form of a rod in the diagonal $A\ C$, its cross section must be equal to $B\ D$ or a, and its effective moment of resistance would be $R\,a^2$. *It appears, therefore, that the power of a plate to resist an angular change of figure in a rectangular frame will be*

two thirds as great as that of an equal quantity of material placed in the direction of a diagonal.

Where a truss is designed to support a constant weight uniformly distributed, or, in other words, where counterbracing is unnecessary, the adoption of the tubular or plate arrangement involves an expenditure of 50 per cent more material than would be required by diagonal rods; but where the load is variable, — as is always the case in bridges, and where rods are consequently required in both diagonals, — the relative economy of the two arrangements is more nearly equal, and may even be reversed. In a truss which is unsupported by an arch, a calculation of the quantities of the material required in the two arrangements involves a consideration of the strains produced by the variable and constant loads in each panel. In the middle panels the strain from the weight of the structure being theoretically nothing, the diagonals resist only the effects of the variable load, and must be equal. If the area of each rod be represented by $\frac{2}{3}a$, the two would be $\frac{4}{3}a$; but as the braces and counterbraces are never both in action at the same time, the plate arrangement requiring a quantity of material represented by (a) would be equivalent to the braces and counterbraces both, and would effect a saving of material equal to $\frac{1}{3}a$; *the rods in this case would require* 33 *per cent more material than the plate.* If $\frac{2}{3}b$ represent the material required in the rods of the end panels to resist the weight of the structure, then $\frac{2}{3}b + \frac{2}{3}a$ would be required for the main diagonal rods, and $\frac{2}{3}a$ for the counterbrace, making $\frac{2}{3}b + \frac{4}{3}a$. The plate arrangement would require $b + a$. The difference is $\left(\frac{b - a}{3}\right)$. Hence, if (a) should be equal to (b), the value of the expression becomes zero; or, in other words, *if the variable and constant loads are equal at the end panels, the quantity of material in the rods and plates will be the same.* But if the variable load is least, there will be a loss of material by the use of the plates which will be greater as the weight of the structure increases; and, consequently, *the relative economy of the plates will be less for very long spans than for shorter ones.* These considerations suggest the substitution of sheet-iron for the rods of wooden bridges, and its adoption will secure the essentially important advantages of simplicity, economy, incombustibility, and durability, if practical difficulties do not arise from the expansion and contraction of the plates

when attached to wooden frames. As the expansion of diagonal rods in wooden bridges is not found to be productive of any injury or inconvenience, it is possible that the slight bending or buckling which must result from the expansion of the iron plates might not lead to any serious consequences. But whether the plan of constructing bridges by covering wooden frames with sheet-iron be practicable or not, the result of the calculation that has been given proves that, with a sufficient number of posts or stiffeners, the vertical plates in tubular iron bridges may be very thin, and still possess sufficient power of resistance. A tubular iron bridge without arches, to be properly proportioned, should have the number or thickness of the top and bottom plates increased, and the side or vertical plates decrease gradually from the ends to the middle.

A tubular bridge in which the plates are of the same dimensions in the middle and at the ends, must be very badly proportioned, and contain a large amount of useless material. If v represent the variable load upon a single panel or interval between two adjacent posts or stiffeners, $n =$ number of panels, and w the whole weight of the structure, then the proportion between the thickness of the vertical plates in the middle and at the ends will be nearly as $v : \frac{nv + w}{2}$. As this proportion may readily be as great as 1 : 5 it follows that a very great saving may be effected by proportioning the thickness of the sheets in each panel to the strains, an arrangement that appears to have been overlooked or neglected in the construction of tubular bridges.

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2. Remarks on Lithography and Lithographic Transfers. By Lieutenant E. B. Hunt, Corps of Engineers, U. S. A.

The cultivators of science have such frequent occasion to make use of lithography in the publication of their results, that I have thought some brief remarks on this art, and especially on the department of copper or steel plate transfer printing, might possess both interest and utility.

When, in traversing any of our chief cities, we observe the frequent

sign-boards of lithographic establishments, often old and rusty in aspect, it is difficult to imagine that Aloys Sennefelder only made the definite discovery of the lithographic process in the year 1799. This art has a savor of antiquity to us, which makes it seem as if more than fifty-four years had transpired since the ingenious but needy dramatist, in laboring to give cheaper currency to his poetic productions, invented this un-Parnassian process of stone-printing. Sennefelder was a man of true genius, and of such earnest and labor-inspiring determination, that he fairly extorted lithography, in nearly all its present forms, from the dark cave of things unknown. The greatest living practitioners of the art scarcely see its capacities more clearly than did the inventor himself, and in all his laborious conquests of poverty and other physical obstacles to his progress, a distinct sense that a great gift to man, a true democratizer of art, was intrusted to his forming and fostering care, gave him a steady courage until his work was complete. Thanks to the labors of D'Offenbach, De Lasteyrie, Engelmann, Ackermann, Lemercier, and others, the infant art, in being propagated from Munich, its birthplace, has also been much improved in many of its details, and has had some important extensions of its sphere of usefulness and capacity. De Lasteyrie's autographic printing, and Engelmann's printing in colors, were great expansions of Sennefelder's invention, while the excellent management of landscape and scenic effects in Ackermann's establishment in London demonstrated a new capacity of the art.

It is not amiss here to say, that, of all artistic inventions, none, not even mezzotinting or the daguerreotype, has so eminent a capacity for being abused as lithography. None has been made so much the tool of cheap scurrility, or mercenary adventure, founded on the bad taste of the community. Chromatic glories on muddy backgrounds of sooty ink have been diffused, under the guidance of an all-penetrating scent for petty profits, into localities where no educational or philanthropic pioneer has ever been; so that brilliant lake-faced Dulcineas and green-haired Corydons glare down from the most sequestered cabin walls, in suspense between the rifle and the red peppers. The execrable drawing and coloring so characteristic of these intensely cheap chromoliths, are enough to arouse the Jacobinic spirit in one to whom art is a thing of sanctities; and unless philosophy imposes its check, the feeling is almost spontaneous that lithography

is a nuisance, which legislation should abate. Yet the fault is really not in the art, but in its abuse; in its perversion to unscrupulous gain, and in the fact that to poverty, which knows nothing of art, even these tawdry desecrations are really a source of enjoyment. And though this enjoyment be on a par with the savage's ecstasy over glass beads, or rank colored calico, it is still enjoyment, which may also tend to awaken some germs of appreciation where otherwise no art sensibility could ever exist. It is an undeniable fact, that lithography has the capacity for producing in thoroughly skilful hands effects of a high order, and some which are peculiar felicities of this art alone. But that this result may be attained, it is indispensable that labor, care, skill, and indeed all the elements of any excellent art should conspire. The artist needs to be such in fact, as well as in name, and the printer must possess an appreciation of the subject printed, and a technical mastery of his business such as is quite too rare, especially among us, where the manufacture of tawdrygraphs has so long formed the staple business of the craft. When we study the finest prints of Ackermann and Lemercier, we see what can be done, or at least a part of the possible; and we cannot but hope that the true art of lithographic design and printing may yet find foothold among us, and that our trade in flagrant chromoliths may discover its legitimate vent in the Feejee Islands. The series of Julien and Lasalle prints which have had so wide a circulation here, though of very unequal merit, show that a correct taste does so far prevail among us, that we may legitimately trace in it the harbinger of a true indigenous art of lithography in the future. In a nation so uniformly cultivated as ours, this art is the natural, and might be made the high-toned, distributor of the best humanities of design among the many who cannot afford to lavish their narrow means on costly prints. I am well persuaded that a thoroughly good selection of subjects, well drawn and printed, would, if sold at the lowest remunerating rates, meet a real want, confer a true benefit on national taste, and every way be a good undertaking. Yet so few good artists on stone are to be found in this country, that the lithographer who should propose to himself this publishing enterprise would, I think, be forced either to import his artists and printers, or to have his work all done in Paris, London, or Vienna. I can hardly conceive that many years will elapse before this select lithographic gallery of design, adapted to the United States, will be really executed

in a style of at least tolerable merit. This would be the best vindication of a truly beautiful art from the bad repute which an immense system of abuse has now so effectively established.

Lithography owes not only its existence, but its possibility, to the fact that several quarries in the vicinity of Munich furnish slabs of a limestone, uniform in texture, apparently compact, yet really having a somewhat open grain. Though other localities furnish stones which could be used, the real commerce of lithographic slabs is limited to the Bavarian quarries, especially Pappenheim and Solnhofen. These furnish stones of ordinary sizes quite cheaply, so that new quarries, which are from time to time announced, must encounter a low market at the start, unless they are able to furnish, in all the requisite perfection, the largest sizes used. The qualities of a good stone are homogeneousness, with freedom from veins, specks, and flaws, a yellowish-white or a pearly-gray color which is uniform, a hard, fine uniform grain, a conchoidal fracture, with a good degree of strength, and a capacity for receiving good grained or polished surfaces, and of being uniformly acted on by acids. The theory of lithography is briefly this. A grained or polished surface of lithographic stone, having a porous structure, absorbs and firmly retains both water and oil, or inks made with oils or fats. Hence, if parts of the stone surface are covered with a drawing in oil or fat, the remaining surface may be wetted without wetting the inked parts; and then, if ink be rolled over the stone, it will be turned from the wet parts, and will adhere on the inked parts. Thus any drawing made with fat ink on the stone can have its lines duly charged with ink for printing, while all the rest of the stone continues clear. Impression after impression can thus be taken off by wetting and inking the stone after each printing. In this process, the stone is merely the solid support for the ink and water to adhere to, and the whole manipulation has for its object to ink the ink lines, and them only. If the stone is used when it is too dry, the whole surface takes the ink and prints, thus totally ruining the drawing.

The lithographic press consists essentially of a frame, on which the bed for the stone is so sustained as to run freely on rollers, being thus drawn by a crank movement under a sharp-edged wooden scraper. The tympan, stretched on an iron frame, is a broad leather cover, which folds down upon and protects the stone and paper by directly

receiving the wear of the scraper. By a lever movement the scraper is lifted off to permit the stone to return to its place, after pulling the impression.

There are three distinct methods of drawing on stone; — first, with the crayon; second, with the pen or brush; third, with the dry point or graver. Crayon drawings are made on grained surfaces, ground with sand but not polished, the quality of the graining requiring to be nicely adapted to the intended subject. The drawing crayons are compounded of some fat basis, usually mutton-tallow and white wax, and of some coloring material, usually lampblack, with some additional ingredients to regulate its consistency. In executing drawings, great care is requisite thoroughly to guard the stone from crayon-dust, finger-marks, saliva, &c., which will be found to print. In pen or brush drawing, a liquid ink is used instead of the crayon, and in a similar manner, the same precautions being observed to protect the stone. The stones, in these cases, are carefully polished. The finer the graining or polish, the more delicate may the drawing be made. The dry point or graver cannot produce the finest class of effects, for lines cut in stone lack the clear, delicate quality which they possess in steel or copper. Yet, with sufficient skill, care, and labor, water-lining, hachures, outlines, &c. can be cut with such delicacy and character as to render extremely fine subjects quite well.

Printing in colors involves the use of two or more stones, with partial drawings on each stone, their number depending on the number of inks to be printed. Each slab has a drawing covering just the parts intended to receive the particular-colored ink used in printing from that stone. Several of the final colors are usually produced by the blending of the overlaying tints. The great difficulty in color-printing for fine subjects is in securing a correct register, or overlaying of the successive color-impressions. If the paper undergoes any sensible change of dimension during these printings, or if there be any lack of care in placing the sheet on the successive stones, a false superposition results, and hence confusion of outline and incorrect coloring. In large sheets these difficulties are much increased. In all these processes, except that of dry-point engraving, the stone, when the drawing on it is complete, is subjected to the action of a dilute acid, which, acting only on the parts of the face not coated with ink, leaves the inked or printing parts in slight relief, the etching being stopped in

time to prevent the relief parts from being eaten away sensibly on their sides. This process is obviously a very critical one; for while its mismanagement totally ruins the drawing, its success makes the printing strong and clear. Its utility lies chiefly in its removing all dust from the stone, and in its giving relief, with consequent clearness of impression, to the drawing.

I come now to those lithographic methods in which the drawing is not executed on stone, but is made on paper, steel, or copper, and is thence transferred mechanically and bodily on to the stone. The process of transfer from an engraved stone on to another polished stone, already alluded to, involves no such important peculiarities as to demand special mention here, it being based, indeed, on the same principles as all other transfer processes. The term *transfer* is applied to a stone on whose polished face the ink from an original writing or drawing, or from an impression on paper, has been so thrown down, as to admit of printing in the same manner as a drawing first made on stone, thus indefinitely reproducing an original. By this means an unlimited number of copies can be procured from an engraved plate, without sensible wear in the engraving. If the engraved surface be small, several transfers may be put on one stone, so as to print several on a sheet, and several such stones can be prepared for printing in a short time, thus presenting peculiar facility for multiplying copies rapidly. Parts from different plates, borders, letter-press notes, views, &c. can be separately printed, and put into new combinations during the transfer process, so as to make almost any desired re-arrangement of materials without re-engraving. Indeed, the piecing together of parts from different plates is a too frequent mode of making new maps, on which no single element except the combination is new. Most school atlases are printed by transfer, and generally, when a greater number of impressions is wanted than the engraved plate would give, either an electrotype reproduction of the plate or a transfer on to stone must be resorted to, or else the plate must be wholly re-engraved as many times as there would be plates worn out.

In respect to the style of work which transfer printing can now give, it is certain that, with equal care, plate printing gives decidedly the best impressions, though it is possible even now, by sufficient care, to print transfer impressions fully equal to the ordinary plate impressions. There can hardly be a doubt that such improvements

will in time be made, as to render the style of transfer printing nearly as strong and delicate as that of plate printing. The existing imperfections are nearly always due to some discoverable omission of care or lack of skill in managing the numerous details which influence the transferring and printing.

Autographic printing is not now much used, at least in this country, though cases frequently occur in which it is very convenient, or even important. In this method, the writing, drawing, music, or other subject for fac-simile, is written or drawn on autographic paper, with autographic ink, just as it is to be printed. This manuscript is then placed face downwards on the stone, and run through the press until the ink firmly adheres, when the paper, by wetting, can be smoothly stripped off, leaving the ink behind. On cleaning the stone, it can be printed as usual. As this process requires for its successful application a careful observance of sundry precautions, and a special preparation both of the paper and the ink, all of which belong properly to the professional lithographer, the cases are rare in which amateurs will find an advantage in attempting the preparation of autographic material for the transferrer. Indeed, the lithographer would generally prefer to write or draw any subject on the stone, rather than to execute it on paper, and then have to make a transfer. For these reasons, I need not enlarge on this method.

In the transfer process as applied to copper and steel engraved plates, the following are the chief points requiring attention. First of all, the plates themselves must be in proper condition, and appropriately engraved for transferring. If it is known from the first, that a plate is to be printed chiefly by transfer, great care should be taken to insure the clear, firm entering of all fine lines into the metal, while the heavy lines should be cut rather shallow, nearly a uniform depth being used for all the work. Very lightly cut lines are apt to be lost in transferring and printing, while very deep ones hold so much ink that, in printing, they smash out and spread, giving a blotched and ragged effect. Water-lining, hachures, machine-ruling, scales, &c., should be engraved firmly, with the lines not too close, as the slightest spreading of close lines causes them to run together and print as a black mass. It is well, when possible, to avoid placing heavy and light parts together, as they are not apt to harmonize in printing. Clear letters, with firm hair-strokes, with open-pointed N's, M's, V's,

and open-top e's, with the heavy strokes not very broad relative to the light strokes, and in general so cut as to give a rather light and uniform effect, will be found to give letter bodies which will print long numbers without filling in or losing hair-lines. By keeping in mind that transfer impressions are printed from a flat surface, and that the lines have almost no relief except what the ink produces, and also that, when lines spread so as to unite, there is no chance of restoring distinctness, an intelligent engraver can generally avoid those failures of adaptation to transferring, which result in blotches or missing lines. Yet this study of adaptation has a greater importance than is generally attached to it, and the routine methods of engravers make a serious obstacle to insuring it proper attention.

In packing plates for transportation, the engraved faces should be well wax-coated, and on this coating a piece of fine paper should be stuck down tightly, and the plate then imbedded in cotton or paper, free from dust, grit, or lumps. The plates should not be accumulated in contact with each other, and but few should be put in one box, and all of these should be so secured as not to be capable of sliding in their beds. When an engraved plate is to be transferred, all the lines of the engraving should be cleaned, washing, if necessary, with spirits of turpentine, or with a solution of potassa for copper plates. All scratches or spots in the plane surface should be removed by coaling in the manner of engravers when removing the burr from engravings, or the burnisher may be used if there be both skill and care.

The ink employed in taking plate transfer impressions requires to be specially prepared for this use, as the effect of variations in its composition will be greatly to modify the success and printing qualities of the transfer. Its consistency must be so fine and soft that it can be forced into all the engraved lines, and yet the transfer must have its lines clear, sharp, and hard, so that at working temperatures the lines will not spread or break. The composition requires to be varied somewhat, according to the work to be printed. It usually consists of lithographic-printing ink (four parts), what is called transfer ink (six parts), mutton tallow (one part), linseed oil (two parts), and weak varnish (two parts). The transfer ink is composed of yellow wax (ten parts), mutton tallow (one part), white soap (three parts), resin (five parts), weak varnish (ten parts), and lamp-black enough to give a sufficient shade, though the less of this the

better. Great care is requisite in all lithographic operations to procure pure materials, as the effect of impurities cannot be calculated. The method of inking plates is either that pursued by the copperplate printer, when the transferrer possesses the requisite skill, or the following, which is the usual mode. The plate is heated over a stove or furnace, until it is hot enough to soften the ink. The ink tampon, firmly held in the hand, is rubbed slowly, with a rocking motion, over the entire engraved surface of the plate, until all the lines are thoroughly charged with ink. Then the plate is carefully wiped with several successive rags, until all the surplus ink is removed from the face; great care being taken not to wipe out too much ink from the lines, but so to manage the quantity left in as to give the best relief to all the lines in the impression.

All things being thus ready, an impression is taken with the ordinary copper-plate printing-press, and in the ordinary manner, though either an unsized China paper or a specially prepared autographic paper is used. The autographic paper is prepared by brushing or sponging over a paper which moisture does not much affect with a coating of starch or paste mixed with gum, glue, isinglass, gamboge, and sometimes other ingredients, in proportions varied by each transferrer. This paper, when smoothly coated and dried, is delicately moistened between damp blotters for printing, and takes a very perfect impression, in which all the small scratches and clouds of the plate are reproduced. Inspection shows if an impression is proper for transfer, as in that case all the parts are clear and perfect, of a light tone of shade, and of uniform character. Impressions more than a week old are not good for transferring.

Having obtained a satisfactory impression, the transfer proper is then effected. A polished stone of the proper size is carefully dried and bedded on a lithographic press, and all things arranged for printing, the transfer impression being in the mean time moistened slightly between damp blotters. The backing papers are so arranged as to bring all parts under the requisite pressure. If several pieces from different plates are to be combined in transferring, the parts are pasted together so as to present the final arrangement. If several copies of the plate go on one stone, they are transferred in succession. When all is ready, the transfer proof is laid face downwards on the stone, the backers and tympan are folded down, and the stone is run under

the scraper two or three times, the pressure being increased each time. By this means the ink lines are made to adhere firmly to the stone. When water is applied freely to the back of the transfer proof, the starch or paste preparation on its face becomes so softened as to permit the easy separation of the paper from the stone, leaving the ink lines transferred bodily on to the stone. China paper detaches itself, and floats off, while autographic paper is readily stripped off. After this the stone is carefully washed until only the ink lines remain. Then it is acidulated, gummed, and thoroughly dried, a short interval being allowed before inking. A weak ink is used for the first inking, and is rolled on very deliberately, the stone being kept well wetted. The stone soon becomes fit to put in the printer's hands, who needs to manage cautiously for the first hundred impressions to avoid injuring the transfer. The first impressions are generally too light or gray, the best impressions being usually the second or third hundred. A good transfer sometimes gives as many as five thousand unimpaired impressions, though usually they become too much worn after about two thousand printings. The printer can, from skill or the lack of it, greatly affect the durability of any transfer, by looking out constantly for incipient defects, and by a uniform, easy manipulation of the ink-roller.

The quality of paper used has a decided effect both on the clearness of the printing and the duration of the transfer. Soft, thick paper, with little or no sizing, is generally used where strength is not important, but a clear, half-sized paper, not highly calendered, prints very well if it contains no improper ingredients, such as alum or plaster. The paper surface needs to have a slight harshness of feel, or tooth for ink, as it is called, else fine lines are apt to be lost. It is only by a careful selection both of paper and of ink, that even a good printer can do justice to a finely engraved plate, while, with every possible aid, a bad printer will produce very imperfect work, the general shade of his impressions running quite uneven, and portions of his transfer being soon filled in or worn away. The printer must maintain a proper temperature of his stone, cooling it with iced water in hot summer days, and having the room well heated in the winter's cold, or the ink of the transfer will be affected, and blotching will result. Also, the regulating of printing pressure presents considerable difficulty, and needs watchfulness. There are many such de-

tails which make the need of an intelligent and energetic supervision one of the first necessities for good lithography.

The process now described is one which, even as it is now practised, must be called eminently useful. Sennefelder himself used it, though quite imperfectly of course; but it is only during the last twenty years that its capacities have been really developed. The rapid improvements it has experienced make it almost certain that, before many years more, it will have become quite perfect and certain in its results. It is now very far advanced in France, — the true home of lithographic art and science, — as the maps of Departments printed by the government fully establish. The plates of the great topographical survey of the interior of France are re-arranged by transfer into excellent maps of the Departments, with special borders and titles, and with full letterpress statistical notes, printed from movable types, and transferred into the proper spaces. In England and Scotland, plate transfer printing is prosecuted as a business, though with what success I have not the means of knowing. In this country, the great amount of transfer from stone or plates on to stone, in making up checks, bills, labels, &c., supplies many shops with petty jobs in one species of transfer, but a few only are engaged in transferring large steel or copper plates. To do this well requires a man to make plate transferring his business, as otherwise, not only will he fail of success, but he will be apt to seriously injure and deface plates intrusted to his handling. Our principal establishments in which plate transfer printing is extensively executed are J. Ackermann's, 379 Broadway, New York; D. McLellan's, 20 Spruce Street, New York; Wagner & McGuigan's, Franklin Square, Philadelphia; and Duval's, Philadelphia. The plates of the Coast Survey Report have been in part printed by each of these establishments, though sometimes the work has furnished very poor evidence of any skill in managing this process. It was by being for the last two seasons assigned to the charge of inspecting the work on these plates, executed by the two first-named establishments, that I was led to such an acquaintance with the subject as to induce me to make this communication.

The following papers were presented, and most of them were read, but no copy of them has been furnished for publication : —

I. MATHEMATICS AND PHYSICS.

1. On the Limit toward which a certain Series Converges. By Dr. Julius Friedländer, of Berlin.
2. Investigations in Analytical Morphology. By Professor Benjamin Peirce, of Cambridge.
3. Illustrations of Cohesion. By Professor Joseph Henry, of Washington.
4. The Conical Condenser, a Telescopic Appendage. By Lieutenant E. B. Hunt, U. S. Corps of Engineers.
5. Account of Experiments on Sound. By Captain Charles Wilkes, U. S. N.
6. On the Velocity of Transmission of Electric Signals along Iron Telegraphic Wires. By Dr. B. A. Gould, Jr., of Cambridge.
7. On some Special Analogies of Structure in the Eastern Hemisphere of the Earth and the Visible Hemisphere of the Moon, with Conjectures as to the Structure and Appearance of those Portions of the Moon which are invisible. By Professor Stephen Alexander, of Princeton, N. J.
8. On some Relations of the Central Distances of the Primary Planets, Satellites, and Rings of the Solar System, of which Bode's Law would seem to be but an Imperfect Expression. By Professor Stephen Alexander, of Princeton, N. J.
9. On Personal Equations in Astronomical Observations. By Dr. B. A. Gould, Jr., of Cambridge.
10. Personal Scale of Astronomical Observers. By Professor Benjamin Peirce, of Cambridge.
11. On a New Method of securing Uniform Circular Motion in the Machinery used in receiving the Registration of Astronomical Observations of Right Ascension. By Professor O. M. Mitchel, of Cincinnati.

12. On the Comparative Precision of the Electro-Chronographic or American Method of Observation. By Dr. B. A. Gould, Jr., of Cambridge.

13. On the Primitive Forms and Dimensions of the Asteroid Planet, the Cause of the Instability of the same, and of the Varieties in the Orbits of the Asteroids. By Professor Stephen Alexander, of Princeton, N. J.

14. Criterion for the Rejection of Doubtful Observations. By Professor Benjamin Peirce, of Cambridge.

15. Some Errors peculiar to the Observer, which may affect Determinations of the Declinations of the Fixed Stars. By Professor John H. C. Coffin, of Washington.

16. Theory of the Action of Neptune upon Saturn. By Professor Benjamin Peirce, of Cambridge.

17. On the Winds of the Coast of the United States on the Gulf of Mexico. By Professor A. D. Bache, of Washington.

18. On the Earthquake of April 29, 1852. By Lorin Blodget, of Washington.

19. On the Distribution of Heat over the North American Continent and the Construction of its Isothermal Lines. By Lorin Blodget, of Washington.

20. On the Subordination of Atmospheric Phenomena, or the Position of the Several Classes with Respect to the Primary Cause or the Initiatory Processes. By Lorin Blodget, of Washington.

21. On the Barometric Pressure in Certain Latitudes and the Existence of Belts of the Low Barometer in the Arctic Regions. By Lorin Blodget, of Washington.

22. On Optical Meteorology. By Professor Joseph Lovering, of Cambridge.

II. CHEMISTRY AND NATURAL HISTORY.

23. On a Remarkable Class of Conjunct Bases, containing Cobalt and the Elements of Ammonia. By Dr. F. A. Genth and Dr. Wolcott Gibbs, of New York.

24. A Geological Reconnoissance of the Arkansas River. By Dr. J. A. Warden, of Cincinnati.

25. The Artesian Well in Charleston. By Rev. P. R. Lynch, of Charleston, S. C.

26. On the Geological Age and Affinities of the Fossil Fishes which belong to the Sandstone Formation of Connecticut, New Jersey, and the Coal Field near Richmond, Virginia. By W. C. Redfield, of New York.

27. On the Effect of the Reclamation of the Annually Inundated Lands of the Mississippi Valley upon the General Health of the Country and the Navigation of that River. By Andrew Brown, of Natchez, Miss.

28. Account of the Recent American Scientific Exhibitions. By Professor S. F. Baird, of Washington.

29. On the Structure and Transformation of Oscillaria Aureliana. By Professor J. L. Riddell, of New Orleans.

III. PRACTICAL SCIENCE.

30. Notice of Bradford's Machine for separating Metals by their Specific Gravity. By Captain Charles Wilkes, U. S. N.

EXECUTIVE PROCEEDINGS

OF THE

CLEVELAND MEETING, 1853.

HISTORY OF THE MEETING.

In consequence of the prevalence of cholera along the avenues of approach to Cleveland, the meeting which should have been held on the third Wednesday of August (the 18th), 1852, was postponed by the Standing Committee, and was finally appointed for Thursday, July 28, 1853. The attendance was not so large as at the previous meeting.

The meetings were held in the Second Presbyterian Church, and in the Rockwell Street Schoolhouse, and continued until Tuesday afternoon, August 2, when the Association adjourned to meet in the city of Washington on the last Wednesday of April, 1854.

The Association were hospitably entertained by citizens of Cleveland on Monday evening, August 1.

The expense of publishing the volume of Proceedings of the Cleveland Meeting was assumed by the Corporation of the city.

The officers elected for the ensuing year were Professor J. D. Dana, President; Professor J. Lawrence Smith, General Secretary; and Dr. A. L. Elwyn, Treasurer. Professor Joseph Lovering was elected Permanent Secretary for three years, his term of service to begin with the next meeting.

RESOLUTIONS ADOPTED.

Resolved, That the following gentlemen be requested to report on the subjects respectively assigned to them, viz.: —

PROF. A. D. BACHE. *On Recent Additions to our Knowledge of the Theory of Tides.*

PROF. JOSEPH HENRY. *On Recent Additions to our Knowledge of the Laws of Atmospheric Electricity.*

PROF. JAMES HALL. *On Recent Additions to our Knowledge of Paleozoic Rocks.*

PROF. J. L. SMITH. *On the Recent Progress of Micro-Chemistry.*

PROF. WOLCOTT GIBBS. *On the Recent Progress of Organic Chemistry.*

DR. JOSEPH LEIDY. *On the Remains of Fossil Reptiles and Mammals in North America.*

PROF. BENJAMIN PEIRCE. *On the Present State of the Theory of Planetary Perturbations.*

DR. W. I. BURNETT. *On Recent Advances in Anatomy and Physiology.*

PROF. LOUIS AGASSIZ. *On the History of our Knowledge of Alternation of Generation in Animals.*

PROF. J. D. DANA. *On the Geographical Distribution of the Lower Animals.*

PROF. L. S. HALDEMAN. *On the Present State of our Knowledge of Linguistic Ethnology.*

DR. B. A. GOULD, JR. *On the Progress and Developments of the Electro-Chronographic Method of Observation.*

Resolved, That DR. B. A. GOULD, Jr. be requested to deliver a biographical notice of the late SEARS C. WALKER at the next meeting of the Association.

Resolved, That all Committees be discharged except the one to memorialize the Ohio Legislature in reference to a geological survey of the State, and that to memorialize Congress for an appropriation to enable Professor O. M. Mitchel to continue his experiments, and

that the name of Dr. B. A. Gould, Jr. be substituted for that of Sears C. Walker (deceased).

Resolved, That the President of this Association be requested to appoint a Committee of five members, to prepare and present, in the name of this Association, a memorial to the joint Library Committee of Congress, urging on it, and through it on Congress, the advantages of establishing a complete, thoroughly organized, and liberally sustained Geographical Department of the Congress Library, and presenting therein such a project or plan of organization for this Department as shall seem to the Committee best adapted to promote its final usefulness and success, in relation both to the government and the country at large.

The names of the members of this Committee will be found in the Introduction to the volume.

Resolved, —

1st. That priority of entry of a paper shall as far as practicable give precedence in its presentation; cases of exception to be decided by the Standing Committees of the Sections.

2d. That if any communication be not ready at the assigned time, it shall be dropped to the bottom of the list, and shall not be entitled to take precedence of any subsequent communication.

3d. That no exchange shall be made between members without authority of the respective Standing Committees.

Resolved, That the names of those only be entered on the list of new members who shall have signified their acceptance of their election.

Resolved, That it be considered just and expedient to pay a sum not exceeding seventy-five dollars to the Permanent Secretary to defray expenses necessary upon attending each meeting, in consideration of the understanding of the Permanent Secretary for the past three years, that his expenses incurred in attending the meetings were to have been paid.

Resolved, That said Secretary be authorized to draw said expenses from the Treasurer.

Resolved, That the edition of the Proceedings of the Cleveland Meeting be 1,500 copies, 500 of which are to be paid for by the Association.

Resolved, That the American Association take a deep interest in the completion of the admirable analytical engine of Charles Babbage, Esq., believing that its results would be of high value in the present condition of applied mathematics and astronomy, and that the practical difficulties to be surmounted in its construction would tend in this, as in Mr. Babbage's difference engine, to the material advancement of the mechanic arts.

Resolved, That a special committee be appointed, consisting of the President of the Association and two others, to communicate this resolution to Mr. Babbage, with the assurance of their sympathy with him in the difficulties which he has encountered.

REPORTS OF COMMITTEES.

Report of the Standing Committee, made August 1st, 1853, on the Resolutions of Dr. B. A. Gould, Jr., introduced July 30th, 1853.

In the opinion of the committee, the American Association was founded for the purpose of bringing together scientific men from different parts of the country, for discussion upon scientific subjects, and for the cultivation of a friendly and social feeling between those devoting their lives to the pursuit of science and those who feel an earnest interest in its progress. It is believed that the organization of the Association had mainly these objects in view; that it was never intended that the Association should accumulate money, or that it should collect by assessment or otherwise more than is sufficient to pay its necessary and legitimate expenses. For some years after the formation of the Association, all expenses connected with its meetings and the publications of its transactions were defrayed by the assessment of its members. The hospitable citizens of Charleston, having extended a cordial invitation to the Association to meet in their city, were not content with overwhelming its members with kindness and attention, but assumed the publication of the transactions at their own expense, and liberally presented an entire edition

of one thousand copies. The example thus set by the city of Charleston has since been followed by almost all those cities in which the Association has held its meetings. In the majority of cases the city has defrayed the whole expense of publishing the transactions, and has presented an edition to the Association. Under these circumstances, the committee considered it in the highest degree indelicate that the Association should speculate upon the generosity of their hosts, and should sell to its members that which has been so freely given. It does not appear to the Committee probable, that the authorities of the different cities have ever intended to make a distinction between the Association as a corporate body and its members as individuals. Yet it is and has been for some time past the practice to sell to members volumes of the transactions, which have not cost the Association a single dollar. This has not been done secretly, but openly and with the consent of the Association, given, it is believed, hastily and without due consideration. The Committee are of opinion, that it forms no part of the object of the Association to accumulate funds for any particular purpose whatever. But with the present system of assessments, funds have accumulated and must continue to accumulate. Independent of the publications of the transactions, the necessary expenditures of the Association need not exceed $ 500 per annum, and might, it is believed, with proper economy, be brought below $ 400. This sum will include the salary of the Permanent Secretary, and the expenses of printing circulars and programmes. The Committee are not aware, under the present system of accepting the hospitalities of cities and towns, that any other expense need be incurred. There are no objects to be obtained by the Association according to its present organization which require a fund. The Association has not adopted the practice prevalent in the British Association, of appropriating pecuniary means to individuals or to committees to aid them in the prosecution of particular investigations. The Committee are not aware that any proposal to do so has ever been brought forward. The majority of the members of the Association are far from being in easy circumstances. The expenses accompanying a residence of a week at the place of meeting are not inconsiderable, and many members can but ill afford to attend the meetings. It appears therefore to the Committee very undesirable to increase those expenses in a wholly unnecessary manner, as is done by the present rate of assess-

ment. The object of the Association in publishing its transactions is not merely to print the papers which are read before it, but to give those papers the widest possible circulation, and thus, in another way, to contribute to the advancement of science. For these reasons the Committee are of opinion that the rate of assessment at present adopted is too high, and that the volumes of the transactions, paid for by the authorities of the places of meeting, should be distributed gratis to the members of the Association, to learned societies, and to the principal public libraries at home and abroad. There are now six hundred paying members of the Association. The income derived from these, at the rate of one dollar for each member per annum, will cover all the necessary expenses of the meetings, and leave a small balance for any purpose unforeseen which may occur. In case, however, the Association should hereafter deem it desirable to publish its transactions at its own expense, it will be necessary to consider the whole subject anew, and to provide for the increased expenditures occasioned by the change. Whether this course be advisable or not, it is not the duty of the Committee to consider. The Committee much regret the want of definite knowledge of the steps taken with regard to the distribution of copies of the Proceedings, &c., and of the number now on hand. The absence of such official information prompts them to offer additional resolutions, calling upon the Permanent Secretary for a detailed report in reference to such property of the Association as has been received and distributed by him without passing through the hands of the Treasurer, and limiting his official action to those matters in which power is distinctly invested in him by the Constitution. With respect to the resolutions authorizing the purchase of copies of Proceedings by the Permanent Secretary, the Committee are of opinion that the copies so purchased should be furnished to members at cost price. For the present the Committee would recommend that the surplus at the end of the present year be retained as a fund, from which expenses may be temporarily defrayed pending the collection of assessments, and that the amount of assessment for any one year should be no more than sufficient to meet the actual expenses of the Association during the year previous. And the Committee would strongly urge upon the Association, that the personal attendance of members upon meetings ought not to increase their yearly dues.

The third resolution referred to the Committee is that of Professor J.

Lawrence Smith, proposing the appointment of a special committee to consider such changes and additions as the Constitution of the Association may require. The Committee cordially recommend this proceeding, and would suggest that this committee consist of ten members, and be so constituted as to represent as equally as possible the several departments of science, with due regard also to the interests of different institutions and geographical sections of the country.

The Standing Committee recommend the adoption of the following resolutions as an expression of the opinions of the Association: —

Resolved, That the Association disapprove of the collection of property with the view to the formation of a permanent fund, and are of opinion that the annual assessment should be as low as practicable for meeting the absolute demand.

Resolved, That in the opinion of the Association the volumes of Proceedings ought to be distributed as widely and on as liberal a scale as practicable, and that, if any special charge be required for the volume, such charge should be decidedly less than the market value of the book.

Resolved, That the Permanent Secretary, with the advice and consent of the President and Treasurer, be empowered to distribute copies of the publications of the Association to learned bodies, or public libraries, in other parts of the world.

Resolved, That, should it become necessary to disburse any funds of the Association at any time when, or at any place where, the Standing Committee cannot be readily convened, the Treasurer, and no other officer, be empowered to do this, upon the requisition of the President and Permanent Secretary.

Resolved, That the vote of Friday last, authorizing the purchase, by the Permanent Secretary and Treasurer, of copies of the Proceedings of a former meeting, be reconsidered, and so far modified as to require these volumes to be furnished to members at cost price.

Resolved, That a Committee of ten be appointed to revise the Constitution, and to report a new draft at the next meeting of the Association.

2. Report of the Committee appointed to memorialize the Legislature of New York in Regard to a Trigonometrical Survey of that State.

The Committee appointed at the Albany Meeting of the Association, to memorialize the Legislature of New York in regard to a trigonometrical survey of that State, respectfully report, that they prepared a memorial, which was taken by a part of the Committee to Albany and presented to Governor Hunt, in February, 1852. It was promptly transmitted to the Senate of the State by the Governor, with a message recommending that measures should be adopted in accordance with the views of the memorialists. No action was taken by the Legislature. At the recent regular session of the Legislature of New York, Governor Seymour recommended in his message a similar survey; but neither at this session, nor at the extra one held since, has any action, it is believed, been taken on the matter. The Committee, having executed the duty assigned to them, respectfully request to be discharged.

A. D. Bache, *Chairman*,
Charles W. Hackley,
O. M. Mitchel,
Elias Loomis,
E. B. Hunt,
Committee.

Cleveland, August 12, 1853.

VOTES OF THANKS.

Resolved, That the Association heartily acknowledge the liberality of the Cleveland, Cincinnati, and Columbus Railroad, the Michigan Central and Southern Railroads, the New York Central, the New York and Erie, the Cleveland and Pittsburg, and the Lake Shore Roads, in remitting the return fare to members of the Association.

Resolved, That the thanks of the Association be returned to the Citizens of Buffalo, for the cordial invitation to the Association to hold its next meeting in that city.

Resolved, That the acknowledgments of the Association be returned

to the Cities of Providence and Nantucket for their respective invitations.

Resolved, That the thanks of the Association be returned to the Curators of the Cleveland Academy of Natural Sciences, for their invitation to visit the Museum of the Academy.

Resolved, That the thanks of the Association be returned to the Citizens of Cleveland, for the hospitable reception and entertainment provided on Monday evening, August 1.

Resolved, That the thanks of the Association be returned to the Trustees of the Second Presbyterian Church, and of the Rockville Street School-house, for the use of these buildings as places of meeting.

Resolved, That the Association accept with thanks the very liberal offer of the Mayor and Council of the City of Cleveland, to publish the Proceedings of this meeting.

Resolved, That the thanks of the Association be tendered to the Local Committee, for the ample accommodation and arrangements which have so much promoted the business of the meeting.

Resolved, That the thanks of the Association be presented to the retiring President, Permanent Secretary, General Secretary, and Standing Committee, for the able and impartial manner in which they have discharged the duties of their respective offices.

STATEMENT OF THE PERMANENT SECRETARY.

When the American Association for the Advancement of Science was invited by the authorities of Cleveland to meet in that City, the expense of printing the Proceedings of that Meeting was generously assumed by the citizens of Cleveland, and a Committee of the Association, consisting of Professors S. St. John and J. H. Smith, was appointed to superintend the publication. A resolution was passed by the Association in acknowledgment of this liberality on the part of the City in which the Association was then convened. For eighteen months the members of the Association waited, patiently or impatiently, for the appearance of this volume, without anything

being heard of it. At the Washington Meeting, in the spring of 1855, the Permanent Secretary was instructed "to inform the Publishing Committee of the Cleveland Proceedings of the disappointment experienced by the members of the Association in not receiving the published Proceedings of the meeting held in that city in July, 1853, and to request this Committee to expedite the publication of these Proceedings."

This resolution was transmitted by mail to the Local Secretary, Professor St. John, but no reply was ever received.

In the mean while many of the papers were published at Cleveland in the *Annals of Science*, conducted by Professor Hamilton L. Smith, a member of the Committee of Publication.

On the 30th of January, 1855, a letter was received from Professor S. F. Baird, late Permanent Secretary of the Association, stating that a copy of the printed papers of the Cleveland Meeting had just been sent to him, with a request to furnish to the printers the introductory matter and Executive Proceedings, necessary to complete the volume. As Professor Baird's term of office had expired, he wished to transfer the business into my hands, advising at the same time that the required matter should be withheld, as the most effectual way of preventing a publication which would be discreditable to the Association. I accepted the transfer, and, after examining what had been printed, and consulting with other members of the Association, I concurred fully in Professor Baird's recommendation to proceed no further with the Cleveland volume until the meeting of the Association at Providence, in August, 1855. The subject of the Cleveland volume was then submitted to the Standing Committee, who sustained the course which had been taken to suppress the volume as printed at Cleveland, and referred the whole subject to a sub-committee, consisting of Professor A. D. Bache, Professor Wolcott Gibbs, and Dr. John Torrey. Their report, with the other documents relating to the same subject, is appended to this statement. This report shows that, in the opinion of those who made it, the Cleveland volume, as first printed, was wholly unworthy of publication, and concludes by advising the careful reprint of a selection of materials. As recommended in this report, a committee, composed of Professor Benjamin Peirce, Professor J. D. Dana, Professor Wolcott Gibbs, and Professor A. D. Bache, was appointed to consult with the Permanent Secretary in regard to the

choice of papers for the new edition, and their advice has been implicitly followed. Before the sheets were sent to press, they were submitted in most cases to the authors for revision; and while in press a proof-sheet was examined by the authors, or, when they could not be reached, by some other competent person. The old wood-cuts, for which the Association had already paid to Brainerd and Burridge one hundred and nineteen dollars and seventy-five cents, have been retained, for the sake of economy, after having been retouched elsewhere at an expense of twenty dollars. The map which accompanies Mr. Blodget's paper, and for which the Association paid the same firm thirty-seven dollars and fifty cents, was rejected, and a new one substituted, towards the cost of which the Association have paid twenty dollars. In addition to all this, the Association have been left to pay the whole expense of reprinting the volume ; and so they have failed, and more than failed, of realizing any advantage from the liberality of the citizens of Cleveland. It was hoped and expected that, by the faithful execution of the engagement made between the Permanent Secretary and Mr. Brainerd, in behalf of Harris, Fairbanks, & Co., the loss on the first edition would be shared between the Association and the printers. In this case, the Association would be in a condition to regard the volume of Proceedings of the Cleveland Meeting, as reprinted, in the light of a gift from the City of Cleveland. But nothing was heard from the parties in Cleveland after the Providence Meeting until I addressed a letter to them asking their decision, and received the reply printed on pages 287, 288, repudiating the contract signed at Providence, and printed on page 287.

As the volumes containing the Proceedings of the Washington and Providence Meetings are already published, and contain the Constitution of the Association and a revised list of its members, I have thought that the additional expense of reprinting *them* also in the Cleveland volume was to be avoided.

JOSEPH LOVERING,
Permanent Secretary.

REPORT OF THE COMMITTEE.

THE Committee to whom was referred the Cleveland volume of the Proceedings of the Association, as printed at Cleveland by Harris, Fairbanks, & Co., report, —

That, having examined the volume, they are of opinion that the errors of the press in it are so numerous, the reports of the discussions so imperfect and even erroneous, the style of printing in many cases so bad, the wood-cuts and some of the other illustrations so poorly executed, the number of papers admitted which should have been excluded so considerable, that nothing less than the total suppression of the volume, and the republication, in proper form, of a selection from it, will be consistent with the character of the Association.

The statements in regard to the mode of preparing this volume are so contradictory,* that the Committee cannot undertake to settle or assign the quarters where blame should rest, and they decline distinctly to do so. They are satisfied with recommending a course which, while it remedies the past action, will not bear hardly upon individuals, the City of Cleveland, or the Association.

Mr. Brainerd of Cleveland, as appears by Paper A, is authorized, in behalf of the printers, to make terms in regard to the Cleveland volume, and the terms which he proposes are stated in Paper B. As the proposition of Mr. Brainerd is, from the nature of the errors in the volume, impracticable, the Committee have substituted that contained in the following resolutions, subject of course to the approval of the Standing Committee. This proposition Mr. Brainerd is willing to accept in behalf of Harris, Fairbanks, & Co.

The Committee also suggest that the sum of thirty-seven dollars and fifty cents be paid to Brainerd and Burridge, for the lithograph of

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* Harris, Fairbanks, & Co., in a letter to me, dated at Cleveland, July 26, 1855, say: "We have had unusual trouble in doing what we have done. There seemed to be no one who would see to the work here. Professor St. John did all he could under the circumstances, reading proofs, &c. But the copy when it reached us was in a bad state, so confused that it was difficult to get any clew to the matter, or the arrangement of the articles. We wrote repeatedly to Professor Baird, but we have never received a reply." "It has seemed impossible to hear from any one connected with the matter who felt interest enough to reply or say a word one way or the other. The proof we have mailed has not even been returned to us." — J. L.

Professor Baird, in a letter to me, dated at Washington, January 30, 1855, says: "I made unceasing efforts to get proof-sheets from Cleveland, but this book [sent January 25, 1855. — J. L.] is absolutely the first sign of a letter I have seen." "I cannot learn that anybody revised the proofs; I wrote to everybody I could think of, asking their intervention to secure proofs." — J. L.

Mr. Blodget's "Chart of the Abnormal Atmospheric Movements on the North American Continent," Mr. Brainerd having made a deduction of fifty per cent on the original charge.

The Committee conclude by submitting these resolutions:—

Resolved, That the Committee on the Volume of Proceedings of the Cleveland Meeting be authorized to propose to Mr. Brainerd, agent of Harris, Fairbanks, & Co., printers, to turn over to the Permanent Secretary of the Association all the printed copies of the Cleveland volume, in order that their publication may be suppressed; and to advance to the same officer, for the use of the Association, three hundred dollars, as damages, and to aid in reprinting the volume; on which conditions, such bills for printing as may be deemed reasonable will be certified by the Permanent Secretary after examination, that the printers may receive their pay from the City of Cleveland.

Resolved, That the Permanent Secretary and four members of the Association, to be selected by the President, be appointed a committee to prepare for publication a revised edition of the Proceedings of the Cleveland Meeting, with authority to make a selection of such papers and proceedings as should appear in the volume, and to print and distribute it as early as practicable.

A.

Professor Brainerd is authorized by us to arrange the matters of the Cleveland Proceedings with the Scientific Association for us. Whatever he may do in the premises will be carried out by us.

Harris, Fairbanks, & Co.

August 10, 1855.

B.

To the Special Committee of the American Association for the Advancement of Science:—

Gentlemen,—Having been authorized by Messrs. Harris, Fairbanks, & Co., printers of the Proceedings of the Cleveland Meeting, to negotiate a settlement for the same, I beg leave to state, that the City of Cleveland, by an ordinance (coupled with an invitation to the

Association to meet in that place), agreed to defray the expenses of printing the minutes of the meeting.

A suitable time having elapsed after the holding of that meeting, Messrs. Harris and Fairbanks received from Professor Baird, I believe, the manuscript of the Proceedings, arranged, classified, and marked with red crayon, indicating the order in which the articles should be *inserted.* The Local Committee at Cleveland had nothing to do in the matter of preparing the copy for the press. Considerable difficulty was early experienced in getting the proof-sheets read, although such proofs were *mailed* to the Permanent Secretary, and to the authors, when their address was known. Some of these have not yet been returned.

Professor St. John (a member of the Local Committee, and Local Secretary I think), upon the urgent solicitation of Messrs. Harris, Fairbanks, & Co., consented to read some of the proofs by copy; others he declined reading, because, as he said, from the very imperfect manner in which the copy was prepared, he could make nothing of it. Some of the matter stood in type three or four months without getting proofs. This, of course, was exceedingly embarrassing to the printers. They have done, I believe, the best they could under all these unfavorable circumstances.

The City of Cleveland is willing to pay the bill for printing, whenever the Association shall have accepted the volume upon any conditions that may be agreed upon. Harris, Fairbanks, & Co. will reprint those papers that are erroneous, if the Association desire it. The adoption of a course something like this will involve the Association in no expense, while it will relieve an individual of a heavy loss.

Respectfully submitted,

J. Brainerd,

For Harris, Fairbanks, & Co.

Providence, August 17, 1855.

The report of the sub-committee was accepted by the Standing Committee; and, with the view of carrying out the resolutions with which the report closes, the following paper was drawn up and signed: —

It is understood between Joseph Lovering, Permanent Secretary of the American Association for the Advancement of Science, on the part of the Association, and Jehu Brainerd, who is authorized to act for Harris, Fairbanks, & Co. of Cleveland, printers, that said Harris, Fairbanks, & Co. shall deliver to the aforesaid Joseph Lovering all the copies of the Proceedings of the Cleveland Meeting of said Association printed by them, in order that the publication of this edition may be suppressed; and that they shall pay to the Treasurer or Permanent Secretary of the Association, for the use of the Association, the sum of three hundred dollars, as damages for the injury received by the Association, and to aid in republishing the Cleveland Proceedings; — in consideration of all which, such bills for printing as may be deemed reasonable shall be certified by the said Lovering, in behalf of the Association, in order that said Harris, Fairbanks, & Co. may receive proper compensation from the City of Cleveland, subject to the acceptance of Harris, Fairbanks, & Co.

Joseph Lovering, *Permanent Secretary.*
Jehu Brainerd,
For Harris, Fairbanks, & Co.

Providence, R. I., August 21, 1855.

Reply of Harris, Fairbanks, & Co. to this Arrangement.

Cleveland, March 27, 1856.

Professor Joseph Lovering, *Secretary of the Scientific Association:* —

Yours of the 1st instant is before us. I hardly know what reply to make to your inquiries. The proposition you returned by Mr. Brainerd was of such a contemptible character that we did not think it worthy of notice. The Proceedings are like the copy, and we forwarded proof after proof, and could get none in return. Then we finished the work, following copy, which we do not doubt was illy prepared, either for the printer or the public. The idea that we should do several hundred dollars' worth of work, and give you three hundred dollars for the privilege, is really ludicrous, if not almost absurd; and we cannot conceive how any man or set of men could make such a proposition.

The Council repudiates the claim, and the Committee have agreed to pay us one half. We think we shall put up the volume and sell or let them have it for the money, if we can do no better.

If we were in fault about this matter, we should not take our present position; but those who have suffered by the delay, in waiting the return of proof, bad copy, &c., know too well what time and trouble were taken to get it into any kind of shape. Professor St. John spent a greater part of the summer in reading proof, and has frequently assured us it was like the copy.

We have always offered, if the errors or "imperfections" as you term them were or could be marked, we would reprint it at our own expense; but if authors wished to amend, or the Association wished to suppress portions which had once been furnished us, we should expect them to act like honorable men towards,

Respectfully yours,

HARRIS, FAIRBANKS, & Co.

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ERRATA.

On page 22, lines 12 and 13, for " screws " read " screens."

On page 55, line 31, for " centre wind " read " storm centre."

On page 153, lines 25 and 26, for " from two hundred and fifty to three hundred " read " from four hundred to four hundred and fifty."

On pages 153 – 156, for " Stone's River " read " Stones River."

On page 154, line 15, for " Trenton " read " middle."

On page 155, for " 6. Litulites undatus " read " 6. Lituites undatus." For " 24. M. suffusiformis " read " 24. M. subfusiformis." Also asterisks in the sixth vertical column should be added, corresponding to 26, 27, 28, 29, and 30.

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